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THE AUTOMATIC GUN AND ITS MILITARY ASPECTS.

BY CECIL HAMELIN TAYLOR.

An automatic gun is one in which all operations to reload and again fire the arm, are executed by energy derived from the discharge of the preceding cartridge.

A gun operated by the rotation of a spindle, manually, by clockwork or by a motor, is not automatic, since the power for operation is originally derived from an external source.

A machine gun is one adapted to be mounted on a support, and in which the handling of the ammunition from a reservoir is done mechanically, irrespective of the source of power. If this power be obtained from the energy of explosion of a preceding cartridge, the arm is an "Automatic Machine Gun," in contradistinction to the "Crank

Machine Gun" operated by turning a crank by hand, and "Motor Machine Gun" operated by an electric or other motor.

The motor gun is confined to naval and militia work and is not in general use. It is nothing more than a crank gun with a motor attached to the driving shaft.

The crank gun has rendered excellent service on many occasions. The Hotchkiss revolving cannon and the Gatling, Gardner and Nordenfelt machine guns are all of this type. They are multi-barrelled guns in which each barrel is fired singly in succession. The rate of fire with a ten-barrelled Gatling gun has attained about twenty shots per second. The cartridges are fed from drums containing several hundred each.

Automatic machine guns are usually single-barrelled, and are fed with a belt holding the cartridges, the mechanism extracting the cartridges from the belt, inserting them into the gun, firing and ejecting the empty cartridge case entirely by the force of explosion generated in the gun itself. These guns have attained a rate of fire of over ten shots per second.

In all varieties of machine guns the size of ammunition may vary from rifle-calibre to full-calibre, throwing a projectile weighing over a pound. Naturally the larger the cartridge used, the larger, heavier and slower is the gun.

Some confusion has arisen in the use of the term "semi-automatic" applied to ordnance. It could be properly used to denote a gun in which any number of its necessary functions are performed automatically, while the remainder are executed by external agents. The term has been applied in two different technical senses to cannon and to rifle-calibre arms. The "semi-automatic" *cannon* is one in which the opening and ejecting (and usually the closing and locking) are done automatically, the charge being inserted by hand. In the "semi-automatic" *small-arm*, all functions except releasing the trigger are automatically performed.

Though it has been but twelve years since the first of the modern small-bore hand repeating rifles was definitely adopted for the armament of the German Army, yet already

the opinion is widely held in military circles that the hand-repeater must shortly be superseded by the automatic small-arm.

In the Reichstag on December 12th of last year (1900), General Von Gossler, referring to the reports that a division of the German Army was to be re-armed, admitted that in order to keep abreast of modern requirements the War office was negotiating with a Swedish inventor for the model of an automatic breech-loader. Thus, as is usual, in military innovations, Germany takes the lead. Already the cavalry of Germany, Italy and Switzerland are arming with automatic pistol-carbines, and experiments with automatic guns are being constantly conducted in France and generally throughout Europe.

The automatic machine gun has been adopted by both services of every great power, and has already done splendid work in service. In our army the Colt gun has been provisionally adopted. The navy department is at present manufacturing automatic full-calibre guns under Maxim's patents, and has, I believe, also a number of Colt guns in service. All existing examples of automatic guns are, however, more or less imperfect, and it is safe to say that no one gun of this nature has demonstrated any marked degree of efficiency over other good examples of its class.

I shall endeavor in this paper to examine into the general theory and practice of automatic gun construction for the purpose of indicating some of the many causes of failure in most of these weapons in the past, as well as the probable lines of the most satisfactory future development.

There are certain broad desiderata for all types and classes of automatic fire-arms: Positiveness, strength, simplicity, compactness and minimum recoil.

The first is positiveness. This is difficult to define correctly. For a mechanism to be positive, as few as possible parts working independently from the main mechanism, and a minimum number of springs should be used. The cartridge from its entrance into the gun until the ejection of the empty case should be held securely at all times. The fundamental mechanical motions of all parts of the mechan-

ism should be such that the maximum frictional coefficients remain low.

This question of positiveness determines to a great extent the limiting amount of abuse through which the arm will remain operative. Under service conditions guns are subjected to severer usage than any other class of similar mechanism. Powder residue and unburned grains of smokeless powder, sand and dust, mud, rust and frozen oil all do their share in clogging the action.

In view of these facts the main problem becomes that of utilizing a fraction of the energy of explosion through as frictionless as possible a mechanism, with a minimum total working surface, in such a way that at any instant a maximum of energy shall be available for overcoming resistances to the cycle of operation. Not only should there be a minimum total working surface, but motion on these surfaces should be as short as possible, and the surfaces should be unbroken, so that foreign matter can not interpose itself in such a way as to place the mechanism in a shearing or crushing position. The speeding of the main pieces, especially of the breech-block, should be such as to give increasing force with lessening velocity in the first part of the opening motion and in the last period of closing, in order to more surely overcome resistance to extraction of the fired case or to the insertion of a live cartridge. The question of compactness and lightness is the last but not the least consideration, and further greatly complicates an already involved problem.

All existing examples of automatic guns designed until recently, have points in common; the block in its cycle is guided by grooves or slots, and a form of firing pin, either primary or intermediate, is used. I estimate from my own experiments and from the detailed official records of trials conducted to simulate service conditions, that fully 90 per cent. of failures to operate under various forms of abuse are directly due to this construction.

The program of trials for military fire-arms generally consists in tests for safety, ease of manipulation, rapidity of fire, ballistics, and ability to resist the action of defective ammunition, rust and dust.

It will be seen that an arm must be satisfactory in *all* particulars, and that failure in any one will condemn it. The question of ballistics alone is independent, generally speaking, from that of the mechanism.

Every fire-arm which can lay any claim to efficiency must be safe, easy of manipulation and compact. The ability, then, to resist defective ammunition, rust and dirt, becomes the subject for investigation.

In the United States trials for selecting a hand magazine arm to supersede the Springfield single-shot rifle, of the fifty-three arms examined, only two passed through the entire trial without breaking or clogging to such an extent that the mechanism worked with difficulty. Twenty-three guns, including models of the best European military rifles now in use, were more or less incapacitated, owing either to rusting or dusting, while four others, after passing through the dust test successfully, were disabled before arriving at the rust test. Of the twenty-three more or less unsuccessful guns, three—one of which was only a single-loader—failed only through rust; seventeen only through dust; while three failed from both causes. The firing-pin caused five failures; the magazine caused four, and the sticking of breech block in its slide occurred in eighteen cases. It is probable that the firing-pin would have given much more trouble than is indicated from these figures, were it not for the fact that in those cases in which the block was permanently jammed the condition of firing-pin was not examined, the failure of the gun being attributed solely to the sticking of the block.

Dangerously defective ammunition is of two kinds: In one the cartridge case becomes ruptured on firing, allowing gas to escape into the mechanism, straining parts and fouling the action. In the other the charges may be either greater or less than normal, giving greater or less breech-pressure. With excessive charges the fermeture, or locking device, is unduly strained, and in automatic guns with reduced charges, the power for operating is lessened.

In the above-mentioned tests of hand repeaters, twenty-seven guns were tested with these forms of defective ammu-

nition prepared for the purpose, the excessive pressure being limited to about 50,000 pounds per square inch. Of these twenty-seven, sixteen passed through the ordeal satisfactorily, and eleven were not repaired to undergo this test. In eight tests the weakened cases caused broken extractors, due to their being unsupported when in the locked position. In each of the twenty-seven guns tested properly placed vents to lead the escaping gases from the cartridge head to the atmosphere would have done much to prevent damage. Supported, strongly made extractors, in addition to the vents, would have entirely prevented it. Firing-pins of all kinds should, where possible, be replaced by some form of swinging hammer, as the effect of rust and dirt can be made much less with this construction than is possible where a firing-pin is employed. When we consider that with modern methods of ammunition manufacture there is only one defective round in about 10,000 to 20,000, we see that resistance to defective ammunition is probably the least of many desiderata for small-arms.

I now come to the real problem of the automatic gun; namely, that of reducing friction and of being able to use the maximum of energy at any instant for overcoming accidental resistances.

Formulae for friction used in machine design cannot be used in gun design, as they are based on a more or less perfect lubrication of working surfaces. Not only does a firearm have to work dry of oil, but, as above mentioned, its hould be usefully operative when cemented with rust and dirt. Under fixed conditions this cementing will then be in proportion to the amount of working surface, the distance travelled on the surface, the tightness of the joints, and the finish and quality of the metal employed.

The first factor is that of the amount of working surface, and the distance travelled by the parts while in contact. There are these four methods of communicating and guiding mechanical motions: (1) by rolling contact, (2) by sliding contact, (3) by wrapping connections, and (4) by link-work. True rolling contact for guiding is practically frictionless. As a power medium it is uncertain, since the

relation of parts in reciprocating movements depends on frictional adhesion, so that the relation of parts may become altered. It also has the disadvantage of becoming a crushing device in the case of dusting. This means may then be considered generally inapplicable to ordnance, and more particularly to small-arm construction. Motion by wrapping connections is really but a form of motion by rolling contact, and should be classed as such. It is open to the same objections and may also be discarded. Motion by sliding contact is obtained by gearing, cams, and pin-and-slot movements. In these ways as a power medium sliding contact is much used, but its use as a guide in the form of a bolt and sleeve construction is universal.

Gears, cams, and pin-and-slot movements may be used in a manner to give fair results. Gears can with difficulty be made to run smoothly without "backlash;" they exert a crushing action on any dirt on their surfaces, and are liable to breakage, owing to the sudden blows to which they are subjected in automatic guns, unless made of a size incompatible with the compactness desired in small-arm construction. For quick-firing ordnance of the semi-automatic type, gearing has been used successfully, as compactness is here not a vital requisite. In cam, and pin and-slot motions, unless rolls be employed, there is much motion and large working surface. Where rolls are employed, simplicity and strength are both sacrificed to a certain extent.

In guiding the bolt by a sleeve we see the result of the neglect on the part of designers to consider the peculiar features of automatic gun construction. The sleeve is mechanically the most perfect way of guiding a reciprocating piston. Strength and a true straight line motion are obtained, and the gun under *steam engine* conditions is ideal, but under military *service* conditions a mallet and drifts are sometimes required to put the piece in operative condition, and by such means parts are frequently injured. Link-work may be used as a powder medium and as a guide, and offers decided advantages over other means. If a breech block can be swung on link-work entirely without other contact than at

the link pins, the total working surface will be reduced to a minimum, as well as the amount of travel on such surface. Further, a pin can be commercially fitted to a bearing with such precision that once oiled or waxed, the entrance of water and dirt will be prevented, without making the mechanism unduly stiff. Direct obstruction to the bearing surfaces cannot occur, and there can be no crushing action except by the block against the breech of the barrel in the closing movement, which action must be common to all forms of guns.

Were it possible to fit a bolt into a sleeve as tightly as I have here suggested the fitting of the link-pins, the bolt would be quite immovable with the amount of energy at our disposal. The best practice is to make the bolt very loose and shaky in its sleeve. This is economy of manufacture, and though it gives plenty of room for dirt to enter, yet also gives plenty for it to leave by. However, the loose fitting of the bolt leads to inaccuracy of shooting, due to irregular whip-action, and varying trigger pull, and occasionally to difficulty of extraction, due to twisting or damaging of shell from its not being regularly and thoroughly supported. This at best is only a means of lessening within a fixed limit, a fault inseparable from the sleeve method of construction.

In a majority of automatic guns, a link action, usually a form of toggle, is used as the power medium. There can, therefore, be no objection raised to it for this purpose; indeed, it is very generally looked upon as the best means yet used. The influences affecting a link action when used solely as the power medium, on a breech-block sliding in ways or a sleeve, likewise affect to the same extent a similar link action acting *both* as driver and guide to a freely moving block. If, therefore, abuse be given a gun of each of these types, the first one will offer the resistance of both link-work and the sliding-block, whereas the second will offer only that due to the link-work. Since I estimate that the resistance to the sliding of the block under varying conditions, and with various constructions, will average, at the lowest, four times that of the link-work, the ratio of resist-

ance between the two types is at least five to one in favor of the link-guided action. Furthermore, the link-guided action would be, generally speaking, cheaper to manufacture, and should allow less rattle and shake to the block.

In order to have the maximum of energy available at any instant to overcome abnormal resistance, it is necessary that the movement of every part should be a function of the movement of every other part. Consider a case in which this principle is not applied—that of the Mauser recoil-operated pistol, for instance. In this arm the block is securely locked to the barrel during a period of recoil, calculated to be sufficiently long to ensure the bullets having left the barrel before the breech is unlocked. During the first period of recoil the barrel and block together move at the same velocity, and since the comparative weights of the barrel and block are about as five to one respectively, the total available energy of recoil stored in them is divided between them in the same proportion. After the unlocking movement, the barrel abuts against a cushion and comes to rest, the block continuing its rearward movement by virtue of the energy stored in it, and with this operates the gun. Thus five-sixths of the total recoil energy is entirely wasted as far as the important operations of extraction, ejection, cocking and insertion are concerned. This is a most important weakness of this arm and is the source of many failures to operate.

There is as yet no recoil-operated arm in which the entire energy taken from the explosion is available for operating at all points in the cycle, but the well known Maxim machine gun and the Borchardt automatic pistol conform to this demand for the first half of the cycle. Take for example the Borchardt pistol (which is but a modified Maxim gun on a small scale). Its operation is roughly as follows: The block is locked home by a toggle joint attached at the rear end to a continuation of the barrel, and at its front end to the block. This constitutes the recoiling system, which slides to the rear in the grip piece, which may be likened to the carriage of a cannon. During the recoil, an arm projecting rearwardly from the rear link of the toggle

is forced downward by contact with a curved surface attached to the stationary grip, thereby raising the central joint of the link and drawing back the block. A long spiral flat spring is so placed as to resist both the recoil and the breaking of the toggle-joint, and at the end of the recoil this spring forces the barrel home and closes the breech, independently of one another.

During the recoil it is plain that if, for any cause the action is prevented from opening, the recoil motion must cease, and that therefore before an obstruction can succeed in stopping the mechanism it must offer sufficient resistance to absorb all the energy acquired by the various pieces from the recoil.

On the other hand, during the return home the movements of the breech-block and barrel are not functionally dependent on one another, and an obstruction to the block's movement is not acted upon by the energy stored in the barrel, which continues home without regard to the position of the mechanism. It is plain that energy stored in springs is not available for overcoming the irregular, sudden resistances met with in automatic gun practice, to such an extent as that stored as energy of movement in the mechanism itself. To give up a given amount of the energy stored in it, a spring must expand a certain amount; if it cannot expand this amount it cannot give up its latent energy. Energy in the form of $M.\ V^2$ is however available with an infinitesimal movement of the part in which it is stored, and it is therefore instantly expended to overcome resistances offered to the movements of the part. This feature, though not possessed to the full amount by any existing recoil gun, yet is common to nearly all forms of gas operated arms, and in my opinion has been one of the chief factors in the comparative success of the latter-type of arm.

A further consideration of small-arm design is that of the magazine. For military use this factor is of extreme importance. For the sportsman a magazine with which he can fire six shots in a few seconds, but cannot fully reload again inside of a minute, is far superior to a single shot rifle with which say thirty shots could be fired within the minute.

But, for the soldier, the second arm would be a more efficient weapon. In other words, it is a rapid continuous fire; rapidity for say 150 shots, which is desired, rather than extreme rapidity for six or ten, though this is also important. For military use, then, the ability to recharge the magazine quickly is of paramount importance, whereas for sporting purposes it is but a secondary consideration.

The ideal magazine for both uses would combine the following points: It should be chargable by single cartridges, or by clip or packet loading, in all positions of the mechanism, and particularly in the "ready-to-fire" position; there should be no lost movement for opening or shutting a trap, as in the Krag-Jörgensen; the magazine should be positive, that is, its operation should not be dependent on a spring; and finally, a single cartridge placed in the magazine should be fed into the chamber by one cycle of operation of the gun. This last consideration is in opposition to a magazine of the Spencer positive type in which, starting with the magazine empty, a cartridge being inserted, the mechanism must be operated as many times as the cartridge capacity of the magazine, before the cartridge enters the chamber. If to these could be added the feeding of cartridges from a belt, the ideal magazine would be attained.

In automatic small-arms, especially in one with a magazine of the above description, a cut-off for reserving the magazine and allowing single-shot fire would be unnecessary. Single-shot fire could be obtained by firing single shots semi-automatically, that is by always releasing the trigger before the second shot, and loading one cartridge into the magazine between each shot. This would give the fastest possible single-shot fire and would keep a full magazine in reserve for emergencies. A cut-off could be used where it is deemed advisable to *compel* slow firing.

One of the debatable problems of machine gun service remains the method of introducing the ammunition into the gun. There are three distinct solutions of the problem: the belt-feed; the drum, or hopper-feed, and the strip-feed.

Their features may be roughly summed up as follows: The belt-feed is probably the most satisfactory, owing to

the ease of manipulation, ability to fire continuously for a great number of shots, lightness, protection to ammunition, and compactness in packing. Its disadvantages are the necessity of more complicated feeding mechanism in the gun, liability to be affected by moisture, and the fact that the cartridges are not usually as securely held as to prevent their occasionally becoming displaced, causing interruption in firing. The Maxim and Colt guns both use the belt-feed.

The strip-feed, being necessarily made in small units, cannot be so easily manipulated, nor can the firing with it be as continuous without an extra man for feeding. It does not protect the ammunition so well as the cloth belt, but it is light, can be packed compactly, and allows the gun mechanism to be greatly simplified. The Hotchkiss gas-piston gun is fed by strips.

The drum, or hopper, feed does not seem to have found favor with designers of automatic guns, though for crank guns it has long been standard. The positive drums, such as the Accles, are bulky and heavy, while the gravity hopper-feeds are uncertain, especially in high-angle firing, one of the first duties of the machine gun of the future. The Carr gun feeds from a gravity drum and the Skoda '93 model from a gravity hopper.

[*To be concluded.*]

ROTARY TRANSFORMERS:

THEIR HISTORY, THEORY AND CHARACTERISTICS.

By George W. Colles, A.B., M.E.

(*Concluded from vol. cli, p. 445.*)

FREQUENCY TRANSFORMATION.

We have now spoken at length of three of the four main kinds[82] of current transformation, so far as they are or may be accomplished by rotary transforming apparatus, to wit,

[82] See page 209 (March number of this *Journal*).

the transformations of species, pressure and phase; it remains to speak briefly of frequency transformation. We shall, however, find very little to say on this subject, because this is a comparatively new and untried field as yet.

Apart from the universal motor generator method, we may outline three general methods or types of apparatus for transforming frequency, the first of which is entirely general, and the other two of limited application and scope. (We do not speak here of the production of high frequency currents, such as are used in induction coils and for producing electric waves, as a matter apart from the subject here considered.) As heretofore observed (page 210*), all apparatus hitherto known for transforming frequency involves moving parts.

In the first or general case just mentioned, the frequency transformation is considered as a generalization of the common *D C* transformation, the latter being, mathematically speaking, an alternating current of zero frequency. If we take a common *D C* generator and rotate the brushes, we shall, of course, obtain an approximately sinusoidal electromotive force, and consequent current, of a frequency corresponding to the ratio of rotation: and, extending this to rotary transformers of any kind, instead of having the brushes stationary upon the *D C* side, we cause them to rotate at the proper speed to give currents of the desired frequency. Three-phase and four-phase (two-phase) currents would, of course, be obtained by three and four brushes respectively, and so on. The desired rate of rotation may be obtained from a separate alternating donkey-motor working synchronously with the primary, and driving the rotating brushes either direct, or, if the ratio of transformation is not a simple one, through the medium of gearing, or it might be obtained through proper gearing from the main shaft of the transformer itself.

This method was first pointed out by Rowland in his patent above referred to (page 436). Obviously it is applicable to any machine provided with a commutator.

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* March number of this *Journal*.

The main practical objection to this method, however, would appear to be great, if not actually insuperable—the common one of sparking. We know that such rotation of the brushes, on a common closed coil *D C* generator, as a means of producing an alternating current, would be wholly impracticable—would, in fact, if any considerable current were drawn from the machine, be productive of violent internal reactions, in addition to the sparking. And we can hardly suppose that the results would be any more favorable on a rotary transformer, where the difficulties with stationary brushes are enough for us to contend with, and often only too great to make a wholly satisfactory machine.

Superadded to this, in the common one-coil rotary transformer, are the two other great objections, first, that it is necessary to just double the complication—to have *two* rotating shafts and *two* sets of slip-rings, instead of one; and, second, the rotating brushes. This latter may be avoided in machines of the inductorium type, because we are not restricted to any particular speed of the rotating commutator, and we may accordingly rotate it at a speed different from that corresponding to continuous current. Yet it is in just these machines that the sparking difficulty is greatest.

This same principle has been applied in a machine invented by Elihu Thomson,[83] wherein the secondary brushes of a dynamotor are made to rotate slowly backwards, so as to produce an alternating current of very low frequency (four cycles per second), adapted to operate reciprocating drills.

The second method of transforming frequency is that embodied in the machines devised by Lieut. F. J. Patten, and hereinbefore referred to (page 273*). These were first brought to public notice by him in 1892, in a paper read by him before the American Institute of Electrical Engineers,[84] and improved forms were shown in 1895[85]. To un-

[83] Patent No. 485,669, of November 8, 1892.

* April number of this *Journal*.

[84] Trans. Am. Inst. Elec. Engs., Vol. 9, p. 66, Feb. 16, 1892.

[85] *Elec. World*, Vol. 26, p. 669, Dec. 21, 1895.

derstand the method we first have reference to the simple form shown in Plate XXVII, *Fig. 1*. Between the poles *NS*

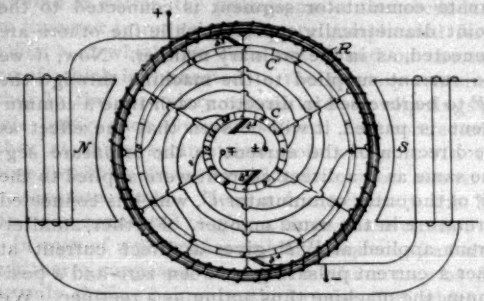

Fig. 1.

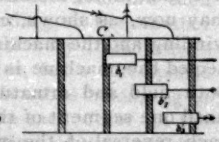

Fig. 3.

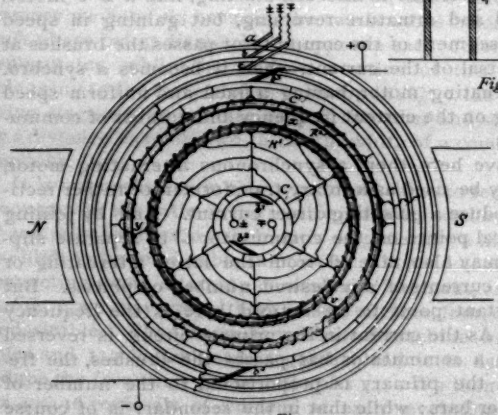

Fig. 2.

PLATE XXVII.—Patten's frequency transformer. 1892.

is placed an ordinary Gramme-wound ring *R*, connected to an ordinary commutator *C* at the centre, provided with the

brushes b^1 b^2; but, instead of the ordinary connection between the commutator segments and those of the ring, every alternate commutator segment is connected to the ring at a point diametrically opposite, while the others are directly connected, as in the ordinary manner. Now, if we suppose the current supplied to the machine through the brushes b^1 b^2 to be reversed in direction each time a commutator segment is passed, it will be seen that the effect as regards the direction of the current in the armature segments is the same as if a direct current were applied to the brushes b^3 b^4 of the outer commutator C', which is connected up to the armature in the usual manner. Further, an alternating current applied at b^1 b^2 gives a direct current at b^3 b^4, or rather a current pulsating between zero and a positive maximum, the machine thus acting as a rectifier. We may now, as shown, connect the brushes b^3 b^4 to the field winding and the machine becomes self-exciting. Thus connected the machine is also self-starting, like a *D C* motor, both field and armature reversing, but gaining in speed until one segment of the commutator passes the brushes at each reversal of the current, when it becomes a synchronous alternating motor, having a fixed and uniform speed depending on the current frequency and number of commutator bars.

We have here, then, a synchronous alternating motor, which may be used as a rotary transformer, or rather rectifier, to produce a pulsating direct current. And by joining symmetrical points on the commutator C' to separate slip-rings, we may also take off from the latter alternating or polyphase currents of any desired number of phases. But the important point to be noticed here is the frequency relation. As the current in the primary circuit is reversed each time a commutator bar passes the brushes, the frequency in the primary is proportional to the number of commutator bars; while that in the secondary is of course proportional to the number of poles. Thus with twelve commutator bars and two poles, as shown in the diagram, *Fig. 1*, the frequency relation is 12 : 2, that is, the frequency of the secondary circuit is reduced in the ratio of 1 : 6.

Of course, however, the alternating currents so produced would be of a very ragged and uneven character. In order to avoid or soften the pulsations in the secondary circuit, the inventor introduces the more complicated form shown in *Fig. 2*, in which two coils are wound on the same core, one of which is connected to the primary commutator, the other to the secondary commutator or slip-rings; the two coils being each Gramme-wound and connected to each other at three symmetrical points x, y, v. In the figure these two coils are for the sake of clearness shown as wound on separate cores. Here a, b, c represent the secondary slip-rings connected to give a three-phase circuit.

In case of a polyphase primary, in place of an alternating, as hitherto supposed, the same machine may be employed, the only change being in the brushes $b^1 b^2$, which are each replaced by two or more brushes, according to the number of phases. The terminals of the stationary transformers are kept separate, so that, for instance, for a three-phase primary we have six brushes, the two brushes belonging to each phase being diametrically opposite each other on the commutator, while the relative position of the brushes belonging to the three phases is shown in *Fig. 3*, the brushes being staggered or set one behind the other and separated by a distance equal to two thirds the width of a commutator segment.

It is not necessary in this type of machine to reduce the frequency in the ratio of the number of poles to the number of commutator bars, as such ratio is generally a high one, too high to be serviceable in practice. Instead of reversely connecting every alternate commutator segment, as shown in *Fig. 1*, we may group them in sets of three, four, or more, as desirable. Thus, suppose we have a machine with four poles and 112 commutator segments, and it is desired to reduce the frequency in the ratio of 1 : 4. We then require $4 \times 4 = 16$ reversals of current at each revolution in the secondary circuit, and as 16 is contained 7 times in 112, we directly connect the first 7 commutator segments, then reversely connect (*i. e.*, to the armature connections on the opposite side of the armature) the next 7 segments, and so on.

There is, however, one practical difficulty in the motor thus far described. It will be seen that at the point of reversal in the primary commutator, owing to the direct and reversed connections, the brushes b^1 b^2 will invariably be short-circuited, owing to the practical impossibility of causing them both to cross from one segment to the next at exactly the same instant; unless the alternative be taken of making the insulation between the commutator segments so wide as to open the circuit for a moment at reversal. As it is, of course, practically impossible to cause the reversal to take place at the mathematical zero point of the current, either alternative causes violent sparking and strain upon the insulation of the circuit. This difficulty is completely overcome by Lieutenant Patten in a later form of machine—though with considerable additional complication—by winding two armature coils in parallel, which are caused to alternate with each other in receiving the current from the brushes, in such manner that there is always sufficient length of wire between the two commutator segments on which the brushes may happen to be to avoid a short circuit, and at the same time there is no necessity of opening the circuit, during commutation.

This type of machine appears to possess great practical merits, along with a versatility surpassed by no other form. According to the statement of the inventor, many machines of this type have been built and in operation, of various forms, and they have worked satisfactorily.

The third and last method of transforming frequency which we shall consider is that due to Steinmetz[86] (Plates XXVIII and XXIX). Referring first to Plate XXVIII, we have at *C* the field and at *S* the armature of a three-phase induction motor *M*. The armature *S* drives, through the belt *B*, the armature of an equal machine *G*, so as to produce three-phase currents; and its own armature, instead of being closed on itself or short-circuited, is carried out into a three-phase external circuit a' b' c' and there made to supply lamps and motors. Now the sum of the

[86] Patents Nos. 620,986, 620,987 and 620,990, of March 14, 1899.

frequency in the circuit $a' b' c'$ of the motor armature and that of the generator armature $a^2 b^2 c^2$—assuming these two

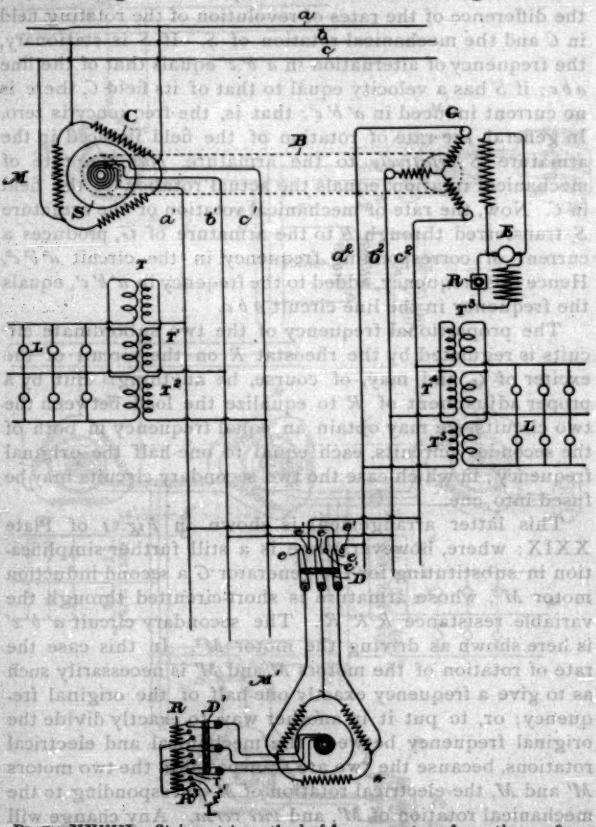

PLATE XXVIII.—Steinmetz's method of frequency transformation. 1899. (First form.)

machines to be alike—is equal to the primary frequency on the supply line *a b c*.

To understand this, consider that the frequency of alternation in the motor armature is equal to the slip—that is, the difference of the rates of revolution of the rotating field in *C* and the mechanical rotation of *S*. If *S* is stationary, the frequency of alternation in $a' b' c'$ equals that of the line $a b c$; if *S* has a velocity equal to that of its field *C*, there is no current induced in $a' b' c'$; that is, the frequency is zero. In general, the rate of rotation of the field induced in the armature *S* ***relatively*** to the armature, ***plus*** the rate of mechanical rotation, equals the actual rotation of the field in *C*. Now, the rate of mechanical rotation of the armature *S*, transferred through *B* to the armature of *G*, produces a current of corresponding frequency in the circuit $a^2 b^2 c^2$. Hence this frequency, added to the freqency in $a' b' c'$, equals the frequency in the line circuit $a b c$.

The proportional frequency of the two subordinate circuits is regulated by the rheostat *R* on the circuit of the exciter of *G*, and may, of course, be anything. But by a proper adjustment of *R* to equalize the load between the two circuits, we may obtain an equal frequency in both of the secondary circuits, each equal to one-half the original frequency; in which case the two secondary circuits may be fused into one.

This latter arrangement is shown in *Fig. 1* of Plate XXIX; where, however, there is a still further simplification in substituting for the generator *G* a second induction motor *M'*, whose armature is short-circuited through the variable resistance $R R' R^2$. The secondary circuit $a' b' c'$ is here shown as driving the motor M^2. In this case the rate of rotation of the motors *M* and *M'* is necessarily such as to give a frequency exactly one-half of the original frequency; or, to put it in another way, to exactly divide the original frequency between the mechanical and electrical rotations, because the two are transposed in the two motors *M'* and *M*, the electrical rotation of *M* corresponding to the mechanical rotation of *M'*, and *vice versa*. Any change will cause a difference of frequency between the secondary circuits of *M* and *M'* to be set up, and tend to re-establish equality; so also with the mechanical work done, the work

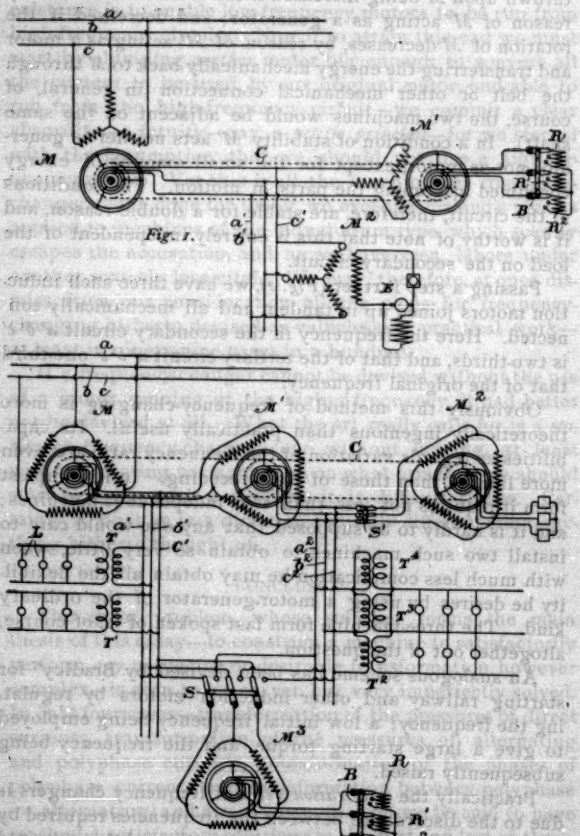

Fig. 2.

PLATE XXIX.—Steinmetz's method of freqency transformation. 1899.
(Second form.)

thrown upon M being increased if the rotation increases by reason of M' acting as a generator; and decreased if the rotation of M decreases, by reason of M' acting as a motor and transferring the energy mechanically back to M through the belt or other mechanical connection (in general, of course, the two machines would be adjacent on the same shaft). In a condition of stability M' acts neither as generator nor as motor, except for the small amount of energy consumed in keeping the parts in motion. The conditions of the circuit, therefore, are stable for a double reason, and it is worthy of note that this is entirely independent of the load on the secondary circuit.

Passing a step further (*Fig. 2*), we have three such induction motors joined up in tandem and all mechanically connected. Here the frequency in the secondary circuit $a' b' c'$ is two-thirds, and that of the tertiary circuit $a^2 b^2 c^2$ one-third that of the original frequency.

Obviously this method of frequency-changing is more theoretically ingenious than practically useful. Its capabilities as regards variation of the frequency ratio are even more limited than those of the preceding. In its simplest form it requires not less than two independent machines; and it is hardly to be supposed that any one would care to install two such machines to obtain so very little, when with much less complication he may obtain all the flexibility he desires by using a motor-generator of the ordinary kind. The three-machine form last spoken of is, of course, altogether out of the question.

An analogous scheme has been devised by Bradley[87] for starting railway and other induction motors by regulating the frequency; a low initial frequency being employed to give a large starting torque, and the frequency being subsequently raised.

Practically the only *raison d'être* of frequency changers is due to the discordance between the frequencies required by ordinary alternating and polyphase motors, and that required for lighting, or rather for economical transformation by

[87] Patents Nos. 615,673, 615,953 and 615,954 of December 13, 1898.

stationary transformers. The be-all and end-all of their existence is to enable low-frequency motors to be run from high-frequency circuits. Now if to attain this end we must start by obtaining *another* motor big enough to convert all the current to be used by our original motor, and able to run from the high-frequency circuit—we commit a clear absurdity—a fatuity—nay, a *petitio principii*—for we started with the assumption that we should not use a high-frequency motor. Yet this is all the inventors of frequency-changers have done for us—if we except Rowland's idea as applied to machines of the inductorium type, which scantly escapes the accusation, and, as we have seen, labors under another scarcely less vital. We may, therefore, at once dismiss from our consideration all the plans for frequency-changing hitherto devised as valueless for practical work—at least in ninety-nine cases in a hundred.

If a frequency-changer cannot be devised without the use of a motor running at the higher frequency, it had better not be devised at all. What the art really calls for is a *stationary* frequency-changer—one without moving, at least without rotating parts; though on what principle we should start to devise such a machine it is difficult to see. Certainly it must be in a direction radically different from anything hitherto thought of.

CONCLUSION.

The great problem, whose solution forms the main thesis of this essay—to construct a machine to satisfactorily perform *any* necessary or desirable transformation, however complex—is seen to be as yet but very imperfectly solved. In only four cases—transformation of the pressures of direct currents, transformation of the pressures of alternating and polyphase currents, transformation of the phases of polyphase currents, and transformation between polyphase (or alternating) and direct currents—can we be said to have reached a satisfactory solution; in the first and last of these, "satisfactory" means only as satisfactory as there is any prospect of getting. Complex transformations are in general difficult and unsatisfactory, requiring more than one

machine. The new type herein called the inductorium offers, however, great hope for the future as regards most of the requirements of electrical energy. As regards frequency transformation, however, so far as practical results are concerned, the art stands where it did in the beginning.

One fact stands out clearly: that practically, the only *raison d'être* of the rotary transformer, dynamotor, motor-generator and all, is the continuous current. Were it not for the continuous current, they would probably have no existence; for wherever only alternating and polyphase currents are concerned, we can at least get along without them, and, except possibly in the *A C–P P C* transformation, most satisfactorily so. Some—the continuous current advocates—by a queer reversal of logic, would lay the blame for this state of facts upon the alternating, instead of on the continuous current where it belongs. Clearly, wherever we have continous-current machinery, there is the ever-present commutator, with all the ills that it is heir to, and that follow in its train—sparking, short circuits, open circuits, overheating, friction, wear, and a delicate sensitiveness to anything like high pressures—and, of course, moving parts. It is vain and useless to speak of the feasibility of continuous current at high pressures; we know that, whatever, according to some, *might* be done, the only fair standard is what *is* done. We know by experience, too, that, whatever we might *a priori* assume, moving parts in electrical machinery—particularly where a commutator is concerned—cannot long be left without supervision. When we have, superadded to the commutator (as in some forms of transforming apparatus) such a thing as *rotating brushes*, the practical difficulties and inconveniences are so great that perhaps 90 per cent. of engineers would prefer any number of slip-rings to even *two* of them. So we have, in the Hutin-Leblanc machine, for instance (page 440), as an alternative to the rotating brushes, a shaft with *forty-eight slip-rings!*

Consider now the matter more fundamentally. In these days when induction plays so great, so all-important a part in electrical engineering, the continuous current labors under the disadvantage of being able to induce nothing

unless by the motion of the circuit itself—which implies mechanical motion. And mechanical motion implies, practically, a commutator and its failings.

Stationary transformation of continuous currents appears for like reasons impossible. By no conceivable method could alternating currents be superposed so as to produce a continuous current; because the *algebraic average* of the strength of every alternating current is zero, and the algebraic sum of several must be likewise zero, while the average strength of a continuous current is, of course, a finite quantity. So far as the pole-changing and rectification methods are concerned, they have, as herein pointed out, proved weak and futile.

Heavily handicapped as it is with its ever-present commutator, the continuous current, as contrasted with the other forms, might be compared to a delicate child. It must never go out in the rain, must be careful not to get overheated, and its strength will not permit it to journey far from home; while at best its life (that of the commutator) is but a short one. But its *good disposition, harmlessness*, and greater *adaptability* keep it in favor.

The alternating current, on the other hand, might be compared to a giant, capable of herculean labors, though somewhat rough, crude and untrained in the finer operations of the arts as yet; of a somewhat uncertain and at times dangerous disposition.

It seems clear that, in the vast undertakings of to day, where the keynote is the *transmission of power*, it is the giant rather than the child of which the world has greatest need. It is possible that the giant may be trained to the docility and susceptibility of the child, but certain that the child cannot be given the strength of the giant.

It does not seem, therefore, to the present writer that the continuous can ever supplant the alternating current for all or even most purposes, as contended by its advocates. And, on the other hand, there are certain cases, in particular electroplating and electrochemistry generally, where the continuous current seems the only one available. Probably we shall continue to need them both, in spite of all our inge-

nious plans to dispense with one or the other. And as long as we need both, so long shall we need to transform one into the other. And as long as we have need of transforming machinery for this purpose, so long, as now made clear, shall we need *rotary* transformers. The art, therefore, which we have just surveyed is not to be considered a transitory one.

ANTARCTICA: A HISTORY OF ANTARCTIC DISCOVERY.*

By Edwin Swift Balch.

(*Concluded from p. 428.*)

III. VOYAGES SUBSEQUENT TO THE DISCOVERY OF A SOUTH POLAR CONTINENT.

The year after Wilkes and D'Urville comes another expedition, that of Sir James Clark Ross,† which confirms the belief that south of Australia is a continental mass of land. The expedition was decided on, in acquiescence to a series of resolutions adopted by the British Association, in August, 1838,‡ and the instructions to Ross from the Lords Commissioners of the Admiralty were dated the 14th day of September, 1839.§ Ross knew all about Wilkes' cruise and the discovery of the Southern Continent before he sailed from Hobart Town, for he had received a long letter from Wilkes on the subject ‖ and also a rough chart. Ross

*Copyright 1900, by Edwin Swift Balch.

† Ross, Captain Sir James Clark, R. N.: "Voyage of Discovery and Research in the Southern and Antarctic Regions," during the years 1839-43; London, John Murray, 1847.

McCormick, R., Deputy Inspector General, R. N., F. R. G. S.: "Voyages of Discovery in the Arctic and Antarctic Seas," etc., London, Sampson Low, Marston, Searle and Rivington, 1884.

‡ "Voyage," etc., Vol. I, page v.

§ "Voyage," etc., Vol. I, pages xxi-xxviii.

‖ Published in Wilkes' "Narrative," etc., Vol. II, pages 453-456, and also in Ross' "Voyage," etc., Vol. I, pages 346-352.

speaks of the action of Wilkes in the following terms:* "I felt therefore, the more indebted to the kind and generous consideration of Lieutenant Wilkes, the distinguished commander of the expedition, for a long letter on various subjects, which his experience had suggested as likely to prove serviceable to me * * * and I avail myself of this opportunity of publicly expressing the deep sense of thankfulness I feel to him for his friendly and highly honorable conduct."

Nevertheless Ross seems to have considered that Wilkes should not have made a cruise to the Antarctic when he, Ross, was coming, for he says:† "That the commanders of each of these great national undertakings should have selected the very place for penetrating to the southward, for the exploration of which they were well aware, at the time, that the expedition under my command was expressly preparing, and thereby forestalling our purposes, did certainly greatly surprise me." This statement is certainly surprising. Ross' expedition only had its *inception* in August, 1838, the very month in which the instructions of the Government of the United States were issued to Wilkes, and in obedience to which he made his cruise to the Antarctic:‡ facts which Ross must have known when he published his book in 1847, as Wilkes had published his book in 1845, and Ross had seen it.

Ross sailed from Hobart Town on November 12, 1840. He selected the meridian of 170° east, "on which to endeavor to penetrate to the southward. * * * My chief reason for choosing this particular meridian in preference to any other was, its being that upon which Balleny had in the summer of 1839, attained to the latitude of sixty-nine degrees, and there found an open sea."§ His ships, the "Erebus" and "Terror," having been thoroughly strengthened, were much more suitable for ice navigation than those of Wilkes or D'Urville. In consequence, he was

* "Voyage," etc., Vol. I, pages 115, 116.
† "Voyage," etc., Vol. I, pages 116, 117.
‡ See *ante*.
§ Ross: "Voyage," etc., Vol. I, page 117.

able to break through the pack, and on January 11, 1841, in 71° 15′ south latitude, "A strong 'land-blink' made its appearance* in the horizon as the ships advanced, and had attained an elevation of several degrees by midnight. All of us were disposed to doubt that which we so much apprehended, owing to its much paler colour than the land-blinks we had seen in the northern regions, but soon after 2 A.M. the officer of the watch, Lieutenant Wood, reported that the land itself was distinctly seen directly ahead of the ship. * * * It rose in lofty peaks, entirely covered with perennial snow; it could be distinctly traced from S. S. W. to S. E. by S. (by compass), and must have been more than one hundred miles distant when first seen. * * * The highest mountain of this range I named after Lieutenant-Colonel Sabine," etc.† The same day Ross christened Cape Adare and Admiralty Range, and the next morning he landed on Possession Island, in 71° 56′ south latitude, 171° 7′ east longitude, "composed entirely of igneous rocks, and only accessible on its western side."‡

Ross worked gradually south. On January 15th he named Mount Herschel; on January 17th Coulman Island; on January 21st, in 74° 15′ south latitude, he named Mount Melbourne; on January 27th he was in 76° 8′ south latitude, 168° 12′ east longitude, and landed on an island which he called Franklin Island. On January 18th, "we stood to the southward, close to some land§ which had been in sight since the preceding noon, which we then called the 'High Island'; it proved to be a mountain twelve thousand four hundred feet of elevation above the level of the sea, emitting flame and smoke in great profusion: at first the smoke appeared like snow drift, but as we drew nearer its true character became manifest. * * * I named it 'Mount

* "Voyage," etc., Vol. I, page 183.

† "This statement of Ross of seeing the "land-blink" at a distance of over 100 miles is interesting, for it proves that Lieutenant-Commander Ringgold may have seen the "loom over high land" over the Balleny Islands on the 13th of January, 1840, on which date he was almost exactly two degrees of latitude north of them. See *ante*.

‡ "Voyage," etc., Vol. I, page 189.

§ "Voyage," etc., Vol. I, pages 216, 217.

Erebus,' and an extinct volcano to the eastward, little inferior in height, being by measurement ten thousand nine hundred feet high, was called 'Mount Terror.'" The eastern cape at the foot of Mount Terror was named "Cape Crozier" and another mountain, a little further south, "Mount Parry."

Ross christened this land "Victoria Land," "whose continuity we had traced from the seventieth to the seventy-ninth degree of latitude."* The coast line of Victoria Land must surely be a continuation of the coast line of Wilkes Land, and as, to make a land, there must be some extension in breadth beyond the coast line, and as the extension of Ross' coast is due south of Wilkes Land, and only some 3 or 4° of latitude distant, the land mass sighted by Ross, therefore, even if the whole place is an archipelago, must be a portion, a *hinterland*, of the land mass sighted by Wilkes.

From the great volcanoes, Ross cruised eastward, reaching his most southerly point, 78° 4′ south latitude, on February 2d; and his most easterly point, 77° 18′ south latitude, 167° west longitude, on February 5th. During this part of the trip, the ships coasted along a perpendicular barrier of ice, some 45 to 60 meters high and more than 700 kilometers long. Ross then started northward, and on the 21st his vessels were again near Cape Adare. They kept on north, and on March 4th, passed well to the eastward of the Balleny Islands, being in 66° 44′ south latitude, 165° 45′ east longitude.† Ross then sailed northward and westward; on March 6th he was in 64° 51′ south latitude, 164° 45′ east longitude, and on March 7th in 65° 31′ south latitude, 162° 9′ east longitude. He then sailed further westward on a track some two degrees north of the track of Wilkes. *He was, therefore, first too far east, then too far north, to see any of the lands sighted by Wilkes himself*, as the South Polar Chart, in the second volume of Ross' book and which gives his track, conclusively proves.

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* "Voyage," etc., Vol. I, page 248.

† "Voyage," etc., Vol. I, page 269.

Ross states, however, that he sailed over a spot, about northeast of the Balleny Islands, which was charted as land on the chart sent him by Wilkes; and on the strength of this only, he did not lay down Wilkes' discoveries on his South Polar chart. Ross devoted many pages of his book to this matter, on which some English geographers lay much stress, and it is necessary, therefore, to discuss it rather at length, and to point out how Ross is in error.*

That Wilkes was justified in laying down this land is evident, for the following reasons: On the chart of the Antarctic Continent, published in the first volume of Ross' book in 1847 only, the Balleny Islands are laid down by Ross himself.† Northeast of them a land is indicated, which is intersected by Ross' track. The chart also gives the position of the "Vincennes on the 13th, and a line drawn from this to the Balleny Islands goes almost through the centre of the land. As the "Porpoise" was close to "Vincennes" on the 13th, the line of vision, in which Ringgold saw the "loom over high land," went straight to the Balleny Isles, which undoubtedly were the cause of this loom.‡ On comparing the statement of Ringgold, with the reported discovery of Balleny, therefore, Wilkes must have considered that Ringgold's appearance of land and the Balleny Isles were the same; and he naturally laid them down on the chart, although a little too far north.

Ross himself quotes § the perfectly straightforward ex-

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*"Sir Clements R. Markham, K. C. B., P. R. G. S., in the ninth edition of the "Encyclopædia Britannica," 1884, article "Polar Regions," says: "At the same time Commander Wilkes of the United States expedition made a cruise to the southward and mapped a large tract of land in the latitude of the Antarctic Circle for which he claimed the discovery. But as a portion of it had been already seen by Balleny, and the rest had since been proved not to exist, the claim has not been admitted."

Mr. W. J. J. Spry, R. N., says ("The cruise of Her Majesty' Ship "Challenger," 1877, page 139): "The supposed existence of this continent was, to a certain extent, proved to be erroneous by Sir James C. Ross' expedition the following year sailing over two of the positions assigned to it."

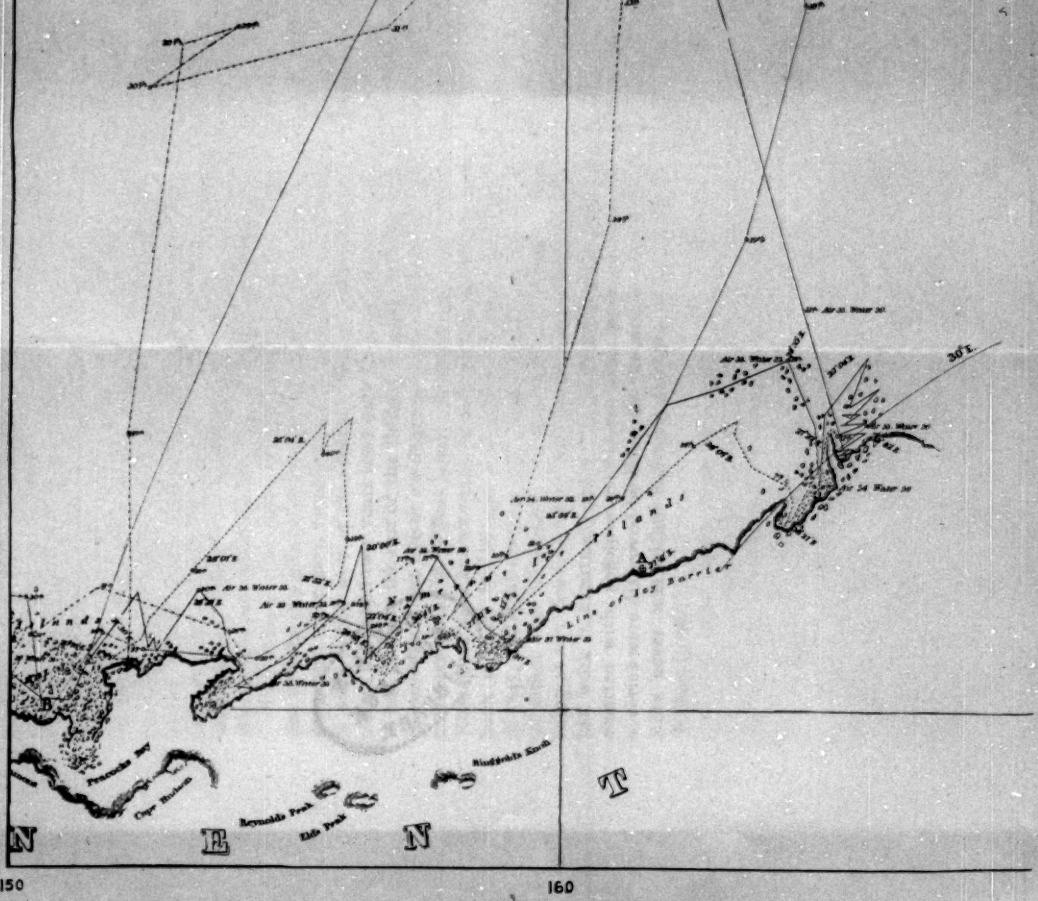
† According to his own statement. "Voyage," etc, Vol. I, page 287.

‡ See *ante*.

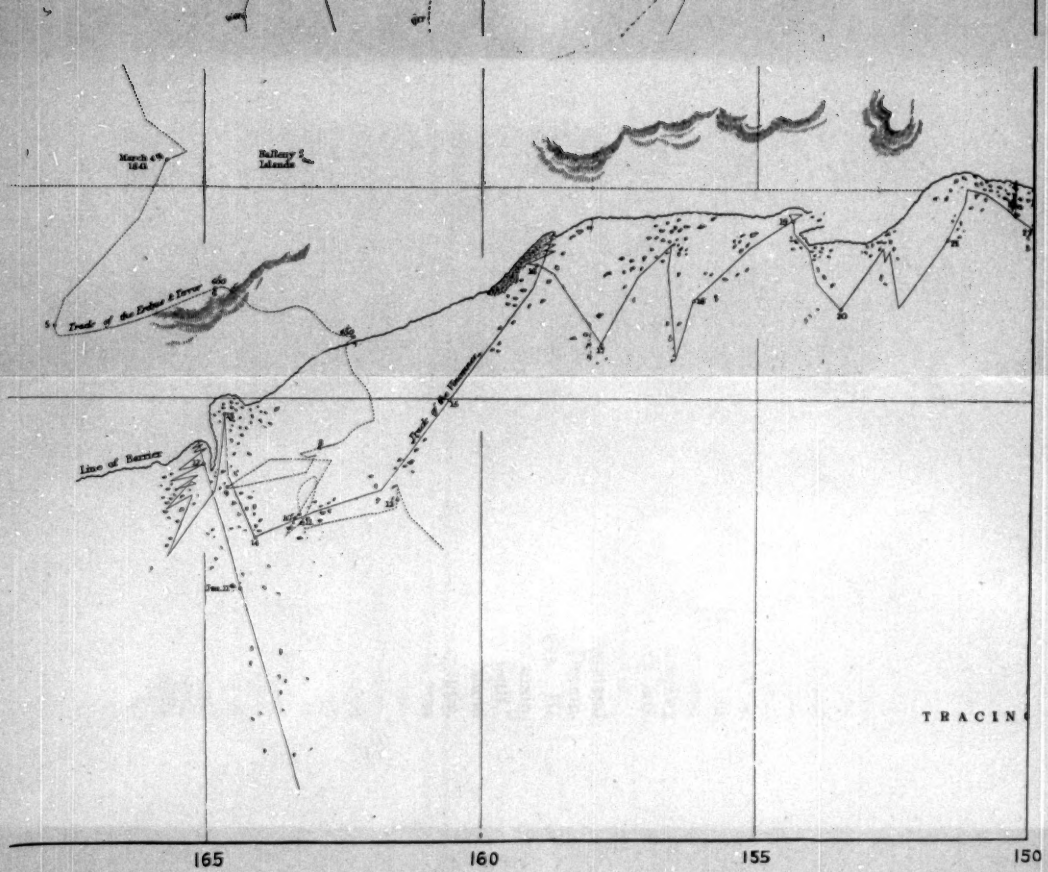
§ "Voyage," etc., Vol. I, pages 286, 287, quoting Wilkes: "Synopsis of the Cruise of the U. S. E. E." delivered before the National Institute in 1842, pages 21 and 26. I have not seen this paper.

planations of Wilkes as to how this land came to be indicated on the chart: "During our cruise, as we sailed along the icy barrier, I prepared a chart, laying down the land, not only where we had actually determined it to exist, but those places in which every appearance denoted its existence, forming almost a continuous line from 160° to 97° east longitude. I had a tracing-copy made of this chart, on which was laid down the land supposed to have been seen by Bellamy (Balleny) in 165° E., which, with my notes, experience, etc., was forwarded to Captain Ross," etc. * * * "As I before remarked, on my original chart I had laid down the supposed position of Bellamy Islands or land in 164° and 165° East longitude, and that it was traced off, and sent to Captain Ross. I am not a little surprised that so intelligent a navigator as Captain Ross, on finding that he had run over this position, should not have closely inquired into the statement relative to our discoveries that had been published in the Sydney and Hobart Town papers, which he must have seen, and have induced him to make a careful examination of the tracks of the squadron, laid down on the chart sent him, by which he would have assured himself in a few moments that it had never been laid down or claimed as part of our discovery, before he made so bold an assertion to an American officer [Captain J. H. Aulick], that he had run over a clear ocean where I had laid down land; and I am not less surprised that that officer should have taken it for granted, without examination, that such was the fact." The fact that Wilkes tried to do justice to the Englishman, Balleny, by recognizing and putting down Balleny's discovery, is unnoticed by Ross. And that Wilkes' explanation is correct, is easily verified by the writings and the charts of the two explorers.

Wilkes in his "Narrative" makes no claim to have sighted any land until in 157° 46′ east longitude, that is several degrees *west* of the Balleny Islands. Even of that he was not quite sure at the time; in fact, not until the observations of the three vessels had been compared, and also because of the more positive proofs of the existence of land afterwards obtained. It was not until January 19th, in



Photographic reproduction of eastern portion of chart published by Lieutenant Charles Wilkes in 1845, showing that the most easterly land which the United States Exploring Expedition thought they had sighted was "Ringgold's Knoll," in 157° 46′ east longitude.



Photographic reproduction of eastern portion of chart published by Sir J. C. Ross in 1847, showing that the northern track of the "Erebus" and "Terror" was eastward of 161° east longitude, and at least 100 miles distant from any part of Wilkes Land.

154° 30′ east longitude, that he "was fully satisfied that it was certainly land." An absolutely indisputable proof that this is true is afforded by the article in the *Sydney Herald* of March 13, 1840, which says that the land was discovered on January 19th, and which twice mentions the longitude as 154° 18′ east. The charts published by Wilkes in 1845, tally exactly with the "Narrative." The most easterly land laid down is "Ringgold's Knoll" in 157° 46′ east longitude, that is, several degrees to the west of the Balleny Islands.

That Ross did not sail over any portion of Wilkes Land can be seen at a glance by comparing the charts of the two explorers. The most easterly land on the charts of Wilkes is well to the *west* of the Balleny Islands; while on the charts published two years later only by Ross, Ross' course is laid down to the *east* of the Balleny Islands, proving that he passed at least five or six degrees to the eastward of the extremest point of Wilkes Land. It would seem as though Ross must have known these facts when he published his book in 1847, for Wilkes had published his "Narrative" in 1845, and Ross mention it.* Still he paid no attention to either the "Narrative" or the charts of Wilkes. One thing at any rate is certain, and that is, that whether Wilkes' work is eventually proved or disproved, yet it may be asserted with absolute confidence that none of his discoveries were disproved by Ross, for the simple reason that Ross never was within sighting distance of any part of Wilkes Land.†

Ross‡ sailed again from New Zealand on November 23, 1841. Icebergs were first seen on December 16th, in 58° 36′ south latitude, 146° 33′ west longitude. Ross en-

* "Voyage," etc., Vol. I, pages 116, 274, 295, etc.

† "The editor of D'Urville's "Voyage" complains forcibly of the treatment meted out to D'Urville in the report of Ross' cruise which he says was published in the *Literary Gazette* of September 16, 1843, and which he thinks either Ross or one of his officers must have written. The editor says ("Voyage au Pôle Sud," etc., Tome Huitième, page 230): "L'auteur de ce rapport, dans l'intention évidente de rapporter à son compatriote l'honneur de la découverte des terres australes, a commis une erreur volontaire et grossière."

‡ "Voyage," etc., Vol. II, pages 125–213.

tered the pack on December 18th, in 60° 50′ south latitude, 147° 25′ west longitude. He worked his way through the pack until February 2d, when the ships were in 67° 29′ south latitude, 159° 1′ west longitude, where he found more open water. On February 9th the ships were in 70° 39′ south latitude, 174° 31′ west longitude. Ross kept working south and on February 16th was in 75° 6′ south latitude, 187° 4′ west longitude, reaching his most southerly point on February 23d, in 78° 10′ south latitude, 161° 27′ west longitude. He was then off the great ice barrier he had seen the preceding year. It averaged thirty meters in height above the water, and soundings in front of it in one place gave a depth of 290 fathoms. There was an "appearance of land" at this spot. Ross then returned, keeping along the edge of the pack until nearly 69° 52′ south latitude, 180° longitude, when the ships ran into open water.

The following year Ross* sailed from the Falkland Islands on December 17, 1842. He met the pack on December 25th, in 62° 30′ south latitude, 52° west longitude. He worked south, and on the eastern coast of the western mainland, charted a large bay as "Erebus and Terror Gulf," and a high mountain as Mount Haddington. A little island east of this, in 64° 12′ south latitude, 59° 49′ west longitude, was named Cockburn Island. On this a small flora was obtained, which Dr. Hooker described.† There were nineteen species, mosses, algæ and lichens. Twelve are terrestrial, three inhabit either fresh water or moist ground, and four are confined to the surrounding ocean. All through January, 1843, Ross beat around in the pack to the east of these lands. He got clear of it on February 4th, when he sailed eastward to try to follow Weddell's track. Between the meridians of 10° and 20° west longitude, Ross pushed south, attaining on March 5th, 71° 30′ south latitude, 14° 51′ west longitude, when pack ice stopped him once more. He then sailed north, and on his return voyage searched in vain for Bouvet Island.

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* "Voyage," etc., Vol. II, pages 321–374.

† "Voyage," etc., Vol. II, pages 335–342.

Capt. William H. Smiley, an American master of a sealing vessel, made a voyage in 1842, probably west of Gerlache Strait. At Pendulum Cove, Deception Island, he found a self-registering thermometer, which was left there in 1829 by Captain Foster. Smiley wrote a letter to Lieutenant Wilkes, who says: * "Captain Smiley, who mentions in his letter to me, that in February, 1842, the whole south side of Deception Island appeared as if on fire. He counted thirteen volcanoes in action. He is of opinion that the island is undergoing many changes. He likewise reports that Palmer's Land consists of a number of islands, between which he has entered, and that the passages are deep, narrow and dangerous."

Lieut. T. L. Moore,† commanding the "Pagoda," sailed from Simons Bay on January 9, 1845. On the 25th, in 53° 30′ south latitude, 7° 30′ east longitude, he met the first icebergs. Then he sailed over the place where Bouvet Island was laid down on the charts, but could not see any land. In 60° 43′ south latitude, 3° 45′ east longitude, he fell in with a singular rock, or rock on an iceberg; the mass of rock was estimated at about sixteen hundred tons; the top was covered with ice and did not appear to have any visible motion, with a heavy sea beating over it; it had a tide-mark round it. On the evening of February 11th, in 67° 50′ south latitude (the highest latitude attained), 39° 41′ east longitude, Moore fell in with heavy pack ice, extending as far as could be seen from the masthead, and the weather becoming thick, he was obliged to work the ship off, being then only 70 miles from Enderby Land. Later they got within 50 or 60 miles, but saw no indications of land. W. D. says of this: "The ship was at one time within 80 miles of Enderby Land; but no indication of such proximity was visible. There were no icebergs nor blink, nor any observable change in the aspect of water or sky."

* "Narrative," U. S. E. E., Vol. I, pages 144, 145.

† "The Nautical Magazine and Naval Chronicle for 1846," London, Simpkin, Marshall & Co., pages 21, 22: "Magnetic Voyage of the 'Pagoda.'" Extract of a letter from Lieut. T. L. Moore, R. N.

"Colburn's United Service Magazine," London, 1850, Part II, pages 101, and the "Antarctic Voyage of Her Majesty's hired barque 'Pagoda,'" by W. D. The author's full name does not appear to be given.

The "Pagoda" continued on an easterly course, encountering comparatively little ice until 64° south latitude, 50° east longitude, where there was a strong ice blink. On March 6th they passed a chain of icebergs and loose ice, and the next morning the ship was surrounded by bergs and pack-ice behind which appeared a high ridge of ice or land, which could only be seen at intervals on the clearing up of the squall, and then only for a short time. From that time the ice got thicker every day; at times more than 100 bergs were seen in a day, one berg being 5½ miles in length and 45 meters high. On March 20th they were driven out of the 6th (*sic*) degree of latitude and 98° longitude by heavy ice, and the appearance of pack ice in the southeast, and thereupon stood northward. Lieutenant Moore says: "In this trip we passed more icebergs than in the three former trips, and likewise have run over more degrees of longitude, inside of sixty, than any ship has done before." The voyage of the "Pagoda" is noteworthy, because of the doubts it throws about the existence of Enderby Land.

Captain William Grant* in the "Day Spring," on December 23, 1855, in 56° 50′ south latitude, 40° west longitude, sighted an icy barrier of flat-topped icebergs, apparently about 120 to 150 meters high, and had some difficulty in extricating his ship from them. There were seldom less than ten or fifteen ice islands in sight until December 27th, in 52° 40′ south latitude, 20° west longitude.

Captain Dallmann,† a German, in the steamship "Groenland," was seal hunting from November 17, 1873, to March 4, 1874. On January 8th, in about 64° 45′ south latitude, Dallmann landed on one of the southern islands of Gerritz Archipelago. On January 10th he found a deep bay, where he appears to have landed in about 64° 55′ south latitude; this bay terminated in a strait which stretched away as far

* Rosser, W. M.: "Notes on the Physical Geography and Meteorology of the South Atlantic," London, James Imray & Son, 1862, page 94.

† "Verhandlungen des Vereins für naturwissenschaftliche Unterhandlung zu Hamburg," Band V, Hamburg, 1883, pages 118–128, 130–136: "Die Entwickelung unserer Kenntnisse der Länder im Süden von America, vom Schiffskapitain A. Schück."

as the eye could see. The land appeared to consist of islands, for Dallmann saw several streaks of blue sky, which seemed as if they must be over straits. The land was high and mountainous, and the coast between the capes was filled with a high upright ice wall, from which large pieces frequently broke off. The strait has been called Bismarck Strait, but a better name would be Dallmann Strait; it is either the southern end of Gerlache Strait or a strait opening into it. Dallmann sailed north from this strait and came to the Shetlands and the Orkneys. He found all the charts extremely unreliable.

In 1874, Captain George S. Nares, R.N., in command of the "Challenger,"* on her deep sea sounding and dredging expedition, after a stop at Kerguelen Island, sailed southeast and crossed the Antarctic Circle. On February 23, 24 and 25, 1874, the "Challenger" was on the outskirts of the pack, reaching 64° 18′ south latitude, 94° 47′ east longitude. The accounts of the different writers disagree in various minor respects, but they agree in stating that the pack was too heavy for an undefended ship to enter, and also that Termination Land was not sighted. The official account is that of Sir John Murray, who says: "After getting clear of the pack at 11 A.M. [25th] the ship sailed along its edge until noon, being from 10 A.M. until that time within about fifteen miles of the supposed position of Wilkes' Termination Land, but neither from the deck nor masthead could any indication of it be seen. The limit of vision as logged was twelve miles, and had there been land sufficiently lofty for Wilkes to have seen it at a distance of sixty miles (which was the distance he supposed himself off

* Report on the Scientific Results of the Voyage of H.M.S. 'Challenger': prepared by Sir C. Wyville Thomson and John Murray; London, 1885; "Narrative," Vol. I, pages 396–452.

Spry, W. J. J., R. N.: "The Cruise of Her Majesty's Ship 'Challenger.'" London, Sampson Low, Marston, Searle & Rivington, 1877.

Campbell, Lord George: "Log Letters from the 'Challenger.'" London, Macmillan & Co., 1876.

Wild, John James: "At Anchor, A Narrative of Experiences Afloat and Ashore during the Voyage of H.M.S. 'Challenger.'" London, Marcus Ward & Co., 1878, pages 59–78.

it) either the clouds capping it or the land itself must have been seen. If Wilkes' distance was overestimated, that of the "Challenger" would be increased, and it may still be found, but as the expression in Wilkes' journal is 'appearance of lands was seen to the southwest, and its trending seemed to be to the northward,' and not that land was actually sighted, and a bearing obtained, it is probable that Termination Land does not exist; still it is curious that pack ice and a large number of bergs should have been found in nearly the same position as by Wilkes in 1840, and this would seem to indicate that land cannot be very far distant."

It is worth noting that Wilkes only speaks of "an appearance of land" at this spot. The most important geographical result of the "Challenger's" southern jaunt, was to prove that there was a floating ice barrier in 1874 in exactly the same situation where there was a floating ice barrier in 1840.

In 1874–75* a party of American, another of English, and a third of German scientists, spent part of the southern summer on Kerguelen Island, principally occupied in making observations in connection with the transit of Venus.

In 1892–1893, four Dundee whalers, the "Active," the "Balaena," the "Diana," and the "Polar Star," made a cruise to the Antarctic.† The ships made no geographical discovery, hunting for seals on the eastern coast of the western mainland, north of 65° south latitude. Mr. Murdoch, an artist, however, made some interesting notes about Antarctic

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*Kidder, J. H., M.D.: "Contributions to the Natural History of Kerguelen Island": *Bulletin of the United States National Museum*, Nos. 2 and 3. Government Printing Office, 1875 and 1876.

†"The *Scottish Geographical Magazine*," Vol. X, Edinburgh, 1894: pages 57–62, "The Story of the Antarctic," by William S. Bruce; pages 62–69, "The Late Expedition to the Antarctic," by Dr. C. W. Donald.

The Geographical Journal, Vol. VII, 1896: "Cruise of the 'Balaena' and the 'Active' in the Antarctic Seas, 1892–93;" pages 502–517, I. The "Balaena," by William S. Bruce; pages 625–643, II. The "Active," by Charles W. Donald, M.D.

Murdoch, W. G. Burn: "From Edinburgh to the Antarctic." Longmans, Green & Co., London and New York, 1894.

color, proving that the Antarctic is not as black as painted. For instance, he says:* "The reader must draw on his fancy for the coloring: the clouds soft warm grey, the crags of the berg to the right a purple lead color, the slope dull white; the berg to the left pale violet, with two or three upright clefts of deep blue, along the top an edge of pure white; between the bergs a third appears light emerald green. The floating ice in front, some parts creamy white, like broken marrons, others dead marble white, and two or three of vivid sky-blue, frosted with white; the sea an umber color, with lavender sheen."

Captain Larsen, a Norwegian sealer, made a cruise in 1892–1893 in the "Jason," on the eastern coast of the western mainland.† The following season, he made a long cruise‡ in the same vessel, landing at Cape Seymour, on November 18th. He says: "When we were a quarter of a Norwegian mile from shore, and stood about 300 feet above the sea, the petrified wood became more and more frequent, and we took several specimens, which looked as if they were of deciduous trees; the bark and branches, as also the year-rings, were seen in the logs, which lay slantingly in the soil. The wood seemed not to have been thrown out of water; on the contrary, it never could have been in water, because, in the first case, we found petrified worms, while there were none in the second. At other places we saw balls made of sand and cement upon pillars composed of the same constituent. We collected some fifty of them, and they had the appearance of having been made by man's hand." These discoveries are noteworthy, for they seem to be the only thing of the kind so far noticed in Antarctica.

From Cape Seymour, the "Jason" first sailed east, then returned and went south along the eastern coast of the

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*"From Edinburgh to the Antarctic," page 286.

† Murdoch, W. G. Burn.: "From Edinburgh to the Antarctic."

‡"The Voyage of the 'Jason' to the Antarctic Regions:" *Geographical Journal*, London, 1894, Vol. IV, pages 333–344.

"*Norske G. S. Aarbog.*" 5. 1893–94, pages 115–131: "Nogle optegnelser af sael og hvalfanger 'Jasons' reise i Sydishavet 1893 og 1894, af Kapt. C. A. Larsen."

western mainland. Larsen christened this coast "King Oscar II. Land" and Foyns Land.* His down track was near 60° and 61° east longitude, and his most southern point, 68° 10′ south latitude, was reached on December 6, 1893.

Captain Evensen,† a Norwegian sealer, in November 1893, cruised in the "Hertha" along the coast of Graham Land. He passed Adelaide Island and the Biscoe Islands, which were almost free from ice, and sighted Alexander Land, which was surrounded by pack ice. He reached 69° 10′ south latitude, 76° 12′ west longitude; the absence of ice at this early period of the southern summer being the noteworthy feature of his voyage.

In 1894–1895, the Norwegian steam whaler "Antarctic" made a cruise to Antarctica.‡ It started from Melbourne on September 20, 1894. On November 6th, they saw such an immense ice island that it was mistaken for land and called Svend Foyn Island. After some cruising, the "Antarctic" reached the Balleny Islands from the north-east on December 14th; then, after much trouble with the ice, on January 16th, Cape Adare; on January 18th, Possession Island, on which several members of the expedition landed; and on January 22d the "Antarctic" was south-east of Coulman Island, in 74° south latitude. On January 23d, the expedition was back at Cape Adare, where a landing was effected, the first on Victoria Land. Some cryptogamic vegetation was found on Possession Island and also

* It is probable that this coast was sighted by Morrell. See *ante*.

† This account is compiled from Dr. Fricker's "The Antarctic Regions," page 128, who quotes Dr. J. Petersen: "Die Reisen des 'Jason' und der 'Hertha' in das Antarktische Meer," 1893–94: Reprint from the "Mitteilungen der Geographischen Gesellschaft in Hamburg," 1895. I have not been able to find this latter paper.

‡ Bull, H. J.: "The cruise of the 'Antarctic' to the South Polar Regions." Edward Arnold, London and New York, 1896.

Borchgrevink, C. Egeberg: "The 'Antarctic's' Voyage to the Antarctic:" *Geographical Journal*, London, 1895, Vol. V, pages 583–589.

Kristensen, Captain Leonard: "Journal of the Right-Whaling Cruise of the Norwegian Steamship 'Antarctic', in Southern Seas: *Transactions of the Royal Geographical Society of Australasia*, Victorian Branch, Vol. XII-XIII, 1896, pages 73–100.

at Cape Adare, and a small number of right whales and many blue whales were seen during the cruise.

Lieutenant Adrien de Gerlache, of the Belgian Navy, in 1898–1899, led an important expedition to Antarctica.* The "Belgica" left Staaten Island on January 13, 1898, sighted the South Shetlands a week later, then crossed Bransfield Strait, and on the afternoon of January 23d was off the coast of Gerritz Archipelago. Here the expedition entered the supposed Hughes Bay, which proved to be the mouth of a new strait, which was called Belgica Strait, and which in size compares with the Strait of "Magalhaens." On the east is a land which was named "Terre de Danco," after a Belgian officer of the expedition, who died on June 5, 1898. On the west is an archipelago, and the action of the Belgians does them credit, for recognizing how much honor is due to Palmer as a discoverer in this portion of the Antarctic, they christened this archipelago, "Archipel de Palmer," and so marked it on their charts.†

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*Cook, Frederick A., M.D.: "Through the First Antarctic Night." New York, Doubleday & McClure Co., 1900. Appendix No. VI of this book: "The possibilities of Antarctic Exploration," touches on the possible political rights of nations in Antarctica; it is undoubtedly the most accurate essay so far written about the Antarctic.

"Société Royale Belge de Géographie:" Bulletin; Vingt-quatrième Année, 1900, No. 1, pages 230. This contains:—

I. "Expédition antarctique belge."

II. G. Lecointe: "Aperçu des travaux scientifiques de l'Expédition Belge."

III. G. Lecointe: "L'hydrographie dans le détroit de 'la Belgica' et les observations astronomiques et magnétique dans la zone australe."

IV. H. Arctowski: "Géographie physique de la région visitée par l'expédition de 'la Belgica.'"

V. E. G. Racovitza: "La vie des animaux et des plantes dans l'Antarctique."

Bulletin de la Société Royal de Géographie d'Anvers, 1900, Tome XXIV, pages 25–51: "Expédition Antarctique Belge," par M. Georges Lecointe.

The Geographical Journal, Vol. XVII, 1901, pages 150–180: "Exploration of Antarctic Lands," by Henrik Arctowski.

†Lieutenant de Gerlache in his recently published papers in the "Société Royale Belge de Géographie," Bulletin, Vingt-quatrième Année, 1900: "Notes sur les expédition * * * aux régions circumpolaires voisines du méridien du Cap Horn," and "Relation sommaire du voyage de la Belgica," pages 365–531, has applied the name of "Gerlache Strait" to "Bel-

Over a hundred islands were discovered in Gerlache Strait, on both sides of which are many peaks, and great ice and snow masses. Many names were bestowed, among which may be mentioned Liège, Gand, Braband, Anvers and Wiencke Islands. The officers made several landings, and many discoveries, and instead of raising flags to take possession of newly-discovered lands, they decided that the first chart of a new country was as good a deed to the title of land, as the formality of pinning a bit of bunting to a temporary post and drinking to the health of a royal ruler. Mr. Arctowski found an insect here, which is probably the first one reported from Antarctica; it was almost microscopic in its dimensions. In about three weeks' time, the "Belgica" sailed without serious difficulty more than three hundred kilometers southwesterly, through Gerlache Strait. To the east the shore line of Danco Land was unbroken; there were many deep indentations, but no passage into the Atlantic. A continuous wall of ice, from fifteen to thirty meters high, fronted the coast everywhere. This land is from 600 to 1200 meters high, with mountains farther inland, perhaps 1800 meters in altitude. Every valley and every surface which was not perpendicular was buried under a sheet of ice. The "Belgica" was unable to follow the coast far enough south to determine whether Danco Land is continuous with Graham Land. On February 13th the "Belgica" was fairly through Gerlache Strait, and for the next few days sailed southwest through an icy ocean, obtaining glimpses of the distant coast of Graham Land. On February 16, 1898, the expedition passed the Alexander Land or Islands, which proved the last land they saw.

De Gerlache now tried to force his way south and west, and succeeded to a certain extent in doing so; but as a result the ship was frozen in and consequently wintered in the pack, from which it was finally liberated in March, 1899.

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gica Strait," and "Gerritz Archipelago," to the islands west of the strait. Both these changes, its seems to me, should be accepted. He also charts the northern coast of the main land as Palmer Land or Trinity Land—and, in my opinion, the first name is the correct one.

The men suffered severely from the absence of sunlight and the lack of fresh food. They were at last driven to eat penguins, whose flesh Dr. Cook describes as appearing to be made up of an equal quantity of mammal, fish and fowl, and as tasting like a piece of beef, an odoriferous codfish, and a canvas-back duck, roasted in a pot, with blood and cod-liver oil for sauce. The furthest southern point, 71° 36′ south latitude, 87° 33′ west longitude, was reached on May 31, 1898. Nothing was seen of Peter I Island, and the course of the ship proved that there was no land near Captain Cook's nor Lieutenant Walker's furthest points.

Perhaps the most important discovery of the Belgian expedition is that of a continental tableland or plateau, situated between 75° and 103° west longitude and 70° to 71° 36′ south latitude. The depth of this continental plateau, from 200 to 500 meters, with an abrupt fall to 1500 meters towards the north, shows that this region has also undergone the depressive movement, which was noticed in the lands of Gerlache Strait. The continental plateau rises gently towards the south, and lowers in its eastern portion towards the north to connect with the continental plateau of Graham and Alexander Lands. It must connect in like manner 50° farther towards the west with the continental plateau discovered by Ross east of Victoria Land. This would show that there is a continuous or uninterrupted continental mass from 50° west longitude, to 63° east longitude, and the discovery made by the "Belgica" gives, therefore, a serious support to the hypothesis of an antarctic continent. The terreous nature of the sediments of the continental plateau and neighboring regions, which contain, besides a grayish slime, a strong proportion of sand, gravel and a number of pebbles of rounded form, are in decided support of this hypothesis. The meteorological observations also show that there must be a great antarctic ice-cap. The minimum temperature,—43°, was observed in September; the maximum, + 2°, in February. The month of July, with an average of — 22·5°, was the coldest of the year; the month of February, with an average of — 1°, was the warmest. The mean temperature of the year was—9·6°,

an extraordinarily low figure for that latitude. This low temperature can only be explained by the absence of land towards the north, and the presence of an antarctic continent entirely covered with ice to the south. The hypothesis is based upon a fact which was observed by the expedition. Every time the wind blew from the north the temperature rose, even in mid-winter, to 0°, but did not ascend higher. As soon as the wind shifted and blew from the south, the thermometer descended abruptly, even in the middle of summer, to a low temperature.

The "Belgica" expedition brought back perhaps more scientific data about the Antarctic than any other expedition so far, and the captain and members deserve the highest praise for their labors.

Professor Chun, of Leipzig, in 1898–1899, led the German deep-sea expedition in the "Valdivia."* Starting from Cape Town on November 13, 1898, on November 25th they sighted Bouvet Island, which was located—finally, let us hope—at 54° 26′ south latitude, 3° 24′ east longitude. Lindsay and Liverpool Islands are probably identical with Bouvet Island, and Thompson Island is perhaps non-existent. Bouvet Island is volcanic, covered with one vast glacier, and no trees were seen through the telescope.

The "Valdivia" then proceeded east and south. The edge of the pack was traced from 8° east longitude to 58° east longitude; the most southerly point reached was 64° 15′ south latitude, 54° 20′ east longitude, when the "Valdivia" was one hundred and two nautical miles from Enderby Land. At this point the enormous icebergs and the strong ice blink to the south proved proximity to land, and it is questionable whether some of the high ice peaks in the far distance did not belong to it. The "Valdivia" came north to Kerguelen Island at the end of December and then left the Antarctic seas.

The scientific results of the voyage are important. The icebergs seen between Bouvet Island and 40° east longitude

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* *The Geographical Journal*, London, Vol. XII, 1898, pages 494–496; Vol. XIII, 1899, pages 297,298, 640–650; Vol. XV, 1900, pages 518–528.

were weather-beaten and carved into grotesque forms; suggesting that they had already made a long voyage and, therefore, that no land exists between 0° and 40° east longitude, except perhaps in high polar latitudes. Between 40° and 62° east longitude, where the icebergs increased, tabular icebergs were found, and the farther east the ship went, the more such tabular bergs did it find. Some of them were to all appearances just broken off the land and showed no clefts. Some rocks, which had dropped from the melting icebergs, were collected in trawls: gneiss, granite, schist and red sandstone, but no volcanic rocks, showing that Enderby Land is not of volcanic origin. This is surprising on account of the soundings made by the "Valdivia." All those between Bouvet Island and Enderby Land were generally over 2,000 fathoms, and the deepest was 3,134 fathoms. This shows that at least between 0° and 50° east longitude, and south of 55° south latitude, there is a fairly regular and deep depression, with no suggestion of a plateau.

Mr. C. E. Borchgrevink led an expedition to Antarctica in 1898-1900.* He struck the ice in 51° 56′ south latitude, 153° 53′ east longitude, then finding the ice conditions unfavorable—which he thinks they always are in this locality—he went east and sighted the Balleny Islands on January 14, 1899. He had trouble with the ice, and was forced northward and eastward. Finally the "Southern Cross" ran into open water and reached Cape Adare on February 17th.

At Cape Adare, Mr. Borchgrevink and the members of his expedition landed and the "Southern Cross" returned north. The expedition spent the winter at Cape Adare in Camp Ridley, making short expeditions in the neighborhood and also scientific observations. Most of the rocks in the neighborhood are of volcanic origin, and represent basaltic lava flows which have taken place during late geological epochs. Six different kinds of lichen were found,

* Borchgrevink, C. E.: "The 'Southern Cross' expedition to the Antarctic: *The Geographical Journal*, London, 1900, Vol. XVI, pages 381–414.

including the ordinary reindeer moss; specimens were obtained as high as 900 meters. In the lichen three distinct types of insect were found: apparently the second discovery of the kind in Antarctica. In Robertson Bay there is also an abundance of fish, and about five different kinds were discovered. August was the coldest month, the mean temperature being —25° 2 C. Many tremendous gales were experienced, the wind sometimes exceeding 90 miles an hour and proving a serious check to sledge expeditions: these gales always came from the same direction, east-southeast.

The "Southern Cross" returned to Cape Adare on January 28, 1900. The expedition then went south, along the coast of Victoria Land. They made a landing in Southern Cross Firth, at the foot of Mount Melbourne, and another at the foot of Mount Terror. From Mount Erebus a smoke cloud was occasionally shot up into the air. The "Southern Cross" then followed the ice barrier eastward until, on February 17th, it reached 78° 34′ south latitude, 164° 10′ west longitude, where a break was discovered in the barrier. Mr. Borchgrevink landed with Lieutenant Colbeck and the Finn Savio, and proceeded southward, reaching 78° 50′ south latitude, the furthest south yet reached. The "Southern Cross" then returned north.*

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*Among valuable papers about the Antarctic may be mentioned the following:

Reiter, Dr. Hans: "Die Südpolarfrage, II. Antarktis, der Sechste Kontinent;" *Zeitschrift für wissenschaftliche Geographie*, Weimar, 1887.

Bartholomew, J. G., F. R. S. E., Hon. Sec. R. S. G. S.: "Antarctic Bibliography;" *Scottish Geographical Magazine*, Vol. XIV, Edinburgh, 1898, pages 563-570.

A bibliography of the papers of the veteran German scientist, Dr. George Neumayer, will be found in Sir John Murray's "The Renewal of Antarctic Exploration," *Geographical Journal*, Vol. III, 1894, pages 40, 41.

ELECTRICAL SECTION.

Stated Meeting, held December 20, 1900.

ELECTROCHEMICAL ACTION.

By C. J. Reed, Member of the Institute.

(*Concluded from p. 413.*)

Nascent State.—One of the most popular fallacies in regard to electrochemical action is the belief that the formation of hydrogen gas at the cathode is a cause of polarization, and that the function of a depolarizer or cathode reagent is to "absorb hydrogen." Hydrogen is sometimes liberated at the cathode as a result of polarization (exhaustion of the proper reagent), but is never the cause of it. If hydrogen is allowed to form, it may act as an obstruction, causing increased resistance, which is very different from polarization.

This fallacy in regard to hydrogen gas seems to be based on the equally absurd belief that water is the only substance capable of undergoing electrochemical decomposition, and that all reduction products except hydrogen, which are formed in an electrochemical cell, are produced by secondary reactions as a result of the reducing action of hydrogen. According to that belief, hydrogen always reduces the cathode reagent or depolarizer, and when the depolarizer is exhausted, the hydrogen appears and "polarizes" the cell. Why hydrogen should have been assumed to be the only substance capable of electrochemical reduction is not apparent, unless it is because water happens to be the cheapest known solvent and happens to be almost universally present and accessible to the circuit in electrolytic solutions.

If we compare the different substances in the table of electrogenic powers, it is difficult to find anything that distinguishes water as the only substance capable of electro-

chemical decomposition. On the contrary, the law of maximum electrogenesis applies to water as well as to any other substance, and any compound, whose electrogenic power is less than 1·48 volts, such as cupric chloride or argentic sulphate, will be decomposed before water is decomposed and before any hydrogen is liberated. Furthermore, it follows from the principle of the conservation of energy, that any substance which has a greater energy of combination than that of hydrogen cannot be decomposed by hydrogen, except by the absorption of external energy. Therefore, hydrogen cannot cause the secondary reduction of any electrolytic conductor in any case of electrochemical reaction, though it may reduce non-conducting substances incidentally present, whose formation heats are less than that of the hydrogen compound. Such secondary reactions, however, are always thermochemical and independent of the electrochemical reaction.

If hydrogen were capable of displacing from a compound an element more highly electrogenic than itself, we should have in that process an inexhaustible source of energy. For example, the combination energy of zinc with oxygen is greater than that of hydrogen with oxygen. Hence, if hydrogen can displace zinc from zinc oxide without absorbing energy, the problem of creating energy is solved without the aid of the philosopher's stone.

But the advocates of the secondary reduction theory say this hydrogen is in the "*nascent state*," and that in this state it has a greater power of reduction than in the ordinary state, and that it can, therefore, reduce zinc, though the combining energy of the zinc is greater than that of the hydrogen. This is assuming, without the slightest evidence, firstly, that the hydrogen performs an impossibility, and secondly, to explain this impossibility, that the hydrogen is in an impossible state.

Let us consider for a moment what this so-called "nascent state" must be. It is defined merely as "the state or condition of an element immediately after it is set free." If it is possible to conceive an atom possessing greater energy in this state than in the immediately preceding or

the immediately succeeding state, without either receiving or evolving energy in passing from one of these states to the other, we can understand the "nascent state."

Hyperergia.—There is another phenomenon resembling polarization, though quite distinct from it, which deserves mention. In the copper-copper-sulphate cell, shown in *Fig. 4*, the continuous passage of a current from an external source in the direction of the arrow will cause an exhaustion of the copper sulphate at the cathode, *C*, by the removal of copper from solution. But at the anode, *C'*, nothing is removed from the electrolyte, and there will be no exhaustion of the copper sulphate, but an increase, due to the formation of additional copper sulphate at that point. The phenomenon of polarization or exhaustion cannot occur, therefore, at the anode, until the copper is exhausted, and there can be no diminution of electromotive force at this electrode through an indefinite continuation of the current. If, however, the strength of the current should be increased until it reaches an intensity corresponding to the total number of molecules of copper sulphate in contact with the electrode at any one time, it is evident that the copper sulphate cannot then transmit a still greater current. If the current be increased beyond this strength, the excess of current must find some other electrolytic path. It must decompose the water present and produce copper oxide or free oxygen. The production of copper oxide is less electrogenic than the production of copper sulphate, and the liberation of free oxygen is electrothanic. An electromotive force is absorbed here and the cell becomes electrothanic. The change in electromotive force caused by this excessive current is similar to the change produced by exhaustion or polarization, except that it is due to a different cause and disappears as soon as the current strength is reduced below a certain limit. There is no specific name for this condition of the cell, but it might with propriety be called *hyperergia*, from *ὑπέρ* and *ἔργον*, signifying an excessively worked condition at either electrode.

There is another cause of variation in the electromotive force of an active electrochemical system, which must not

be mistaken for polarization. This is thermoelectric action, which depends upon temperature relations and current strength, but is not caused or increased by exhaustion of the reagents.

Electrolytic Separations.—We have already referred to the fact that the electrolytic separation of metals is governed by and dependent upon the law of maximum electrogenesis. Its application is very simple and we shall consider briefly two different cases. In the first case, let us suppose a separation is to be made of a mixture of several metallic salts in solution, such as the bromides of copper, nickel, cadmium, and zinc. In order to separate these metals from solution, electrodes of platinum may be used, which introduce no impurities.

When an electric current is passed through the mixture of metallic bromides between platinum electrodes from a governable external source, bromine will be separated at the anode and the metals at the cathode.

From the table of electrogenic powers we find that the separation of cupric bromide requires 0·86 volt; nickel bromide, 1·56 volts; cadmium bromide, 1·64 volts, and zinc bromide, 1·97 volts.

It is evident that as long as the electromotive force of the cell remains above 0·86 volt and below 1·56 volts, copper alone will be deposited at the cathode, and bromine at the anode. As the density of the copper bromide in the solution diminishes, either the current must diminish or the electromotive force must increase. In order to prevent the deposition of any of the other metals present, the electromotive force must be kept below 1·56 volts, and, as the exhaustion of the copper proceeds, the current must continue to diminish until, with the last traces of copper, it becomes infinitesimal. The time required for the complete separation would be infinite. In other words, it is impossible to completely remove the copper under these conditions without removing some of the nickel, though it may be reduced to a high degree of exhaustion after a long time.

When this exhaustion has been reached, the electromotive force is increased above 1·56 volts, but not above 1·64

volts. The strong current now flowing rapidly removes most of the nickel together with the residual copper. When the exhaustion of the nickel has proceeded to a sufficient degree, the electromotive force is increased above 1·64, but kept below 1·97 volts, until the cadmium has been removed. Finally, the electromotive force is increased above 1·97 volts and the zinc removed. The current may now be maintained at any strength and all traces of the several metals entirely removed. But copper, the first metal removed, will be the only one free from other metals. Each metal after the first will be contaminated with a small portion of the preceding metal and probably with more than one.

In practice there are always conditions present that greatly modify these results. For example, the bromides above named could not be mixed together in an electrolyte without either fusion or the presence of a solvent, such as hydrogen bromide or water, which would undergo decomposition and liberate hydrogen when the proper electromotive force is reached. If the fused bromides were used, the electromotive forces would have to be determined for the high temperatures required for fusion. If the solutions have a high resistance, the prescribed electromotive forces may be unable to send a current of sufficient density to prevent recombination by thermochemical action of the deposited metal and the bromine, unless the products are kept separated by a porous partition.

Very different results are obtainable, however, in the case of a mixture of metals instead of a mixture of solutions of metallic salts. In this case one metal can be separated in a state of purity at the cathode, while those which are electro-positive to it (more electrogenic) are left in solution, and those which are electro-negative to it (less electrogenic) are left on the anode. Let us suppose, for example, that metallic copper is to be freed from silver, gold, iron and zinc.

For the electrolyte we use in this case an aqueous solution containing a pure copper salt, such as the sulphate. This solution is not deprived of any of its original quantity

of copper during the process, but is used only as a medium, through which copper is transferred from the impure anode, comprising the mixture of metals, to the cathode, which generally consists of a sheet of pure copper. A strong current of constant strength may here be advantageously employed. The smallest electromotive force is sufficient to send a current through this agenic cell, depositing copper at the cathode and dissolving it from the anode.

From the table of electrogenic powers we find that the electrochemical electromotive force of copper sulphate is 1·21 volts and that of silver sulphate, 0·47 volt, while that of gold sulphate, if it forms a sulphate under the conditions, is much less and probably negative. Hence, as there is neither gold nor silver in the solution, the silver cannot be dissolved without an electromotive force exceeding the difference between 1·21 and 0·47 volts, or 0·74 volt, and the gold cannot be dissolved without a still greater electromotive force. But the copper, zinc and iron all freely dissolve by the electrochemical deposition of the copper at the cathode with any electromotive force greater than zero. The electrogenic power of ferrous sulphate is 2·02 volts, and that of the zinc sulphate 2·30 volts. Hence, iron will not be deposited if the electromotive force between the terminals is less than the difference between 2·02 and 1·21, or 0·81 volt, and zinc will not be deposited if the electromotive force is less than 2·30—1·21, or 1·09 volts.

The law of maximum electrogenesis acts at both terminals of the electrolyte, preventing the silver and gold from dissolving at the anode and preventing the deposition of the iron and zinc at the cathode. Assuming the figures in the table to be accurate, we may feel reasonably certain that, in the refining of copper by this process, the copper product will be free from silver and gold, if the electromotive force is kept below 0·74 volt per cell; that it will be free from zinc and iron, but may contain silver, if the electromotive force is between 0·74 and 0·81 volt; that it will be free from zinc, but may contain silver and iron, if the electromotive force is between 0·81 and 1·09 volts. We may also have the liberation of hydrogen if the electromo-

tive force exceeds 1·48 — 1·21, or 0·27 volt. But so long as the quantity of copper sulphate at the cathode is in excess of the amount necessary to transmit the total current, there will be no deposition of either iron, zinc, hydrogen, or any substance more highly electrothanic than copper, even though the electromotive force may be greatly in excess of that required for such separations, since, by the law of maximum electrogenesis, the reaction absorbing the least electrical energy must be exhausted first. If, for example, the electrolyte be put into an elongated form, so that its total resistance is 100 ohms and an electromotive force of 10 volts be employed, the total current through the cell cannot exceed $\frac{1}{10}$ ampère. If at the same time the solution of copper sulphate at the cathode is very strong and the area of the cathode is not less than 5 or 6 square inches, there could be no deposition of hydrogen, iron or zinc, although the electromotive force is nearly ten times that required to deposit any of these substances under other conditions. Under the conditions named, the high electromotive force applied to the cell would not be absorbed in the electrochemical reaction, that is, in doing the work of chemical separation, but in generating heat in overcoming the resistance of the electrolyte. Under such conditions the electrolyte surrounding the cathode must be kept in motion by other means than diffusion, or the exhaustion of the copper sulphate, even to a slight distance, will reduce the current, causing part of the high electromotive force to be applied to the electrochemical work at the cathode, instead of to the resistance. When this occurs, the hydrogen, iron, and zinc will be deposited. This is always avoided in the practical refining of copper by using a cell of such a form that its resistance is small, in order that the electromotive force may always be kept below the limit required to deposit any objectionable metal.

Changes in Concentration.—It has been stated above that, in the electrolysis of copper sulphate between copper electrodes, the electrochemical action causes the solution to become weaker in copper sulphate at the cathode and stronger at the anode. This and all similar changes in con-

centration, caused by electrochemical action, may be explained very satisfactorily without the necessity of much theory. In *Fig. 8* let *C* and *C'* represent two copper electrodes, the small circles, *c*, representing atoms of copper. Let us suppose that these eletrodes are connected by the solution containing molecules of copper sulphate. Let each molecule of copper sulphate be represented by two circles connected by a line, the circle marked *c* representing an atom of copper, and the circle marked *s* representing the

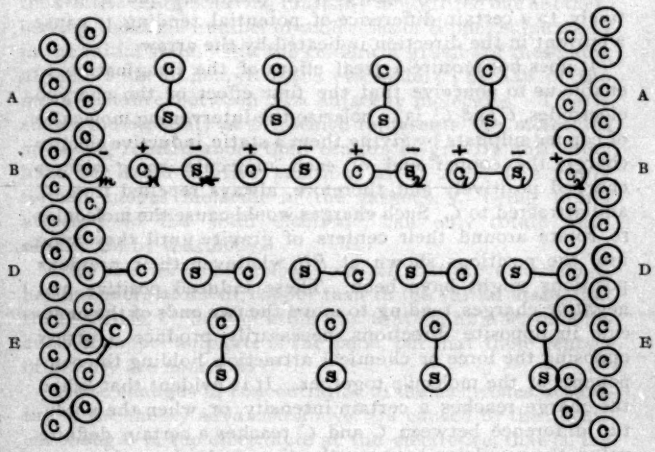

FIG. 8.

$SO_4^{''}$ group of atoms. The line connecting these two components of the molecule in the diagram represents the chemical affinity or attraction that holds the molecule together.

Confining our attention for the present to the upper part of the figure, marked *AA*, we have between the electrodes four molecules of copper sulphate uniformly distributed. It is not necessary to assume that these molecules have or have not any particular arrangement or positions.

Nor is it necessary for us to assume that they have or have not any motion, either of rotation or translation. It is only necessary, for our purpose, to suppose that these molecules are free to move or rotate under the influence of impressed forces. We are at liberty, however, to assume, merely for convenience of illustration, that they are uniformly distributed and arranged in a regular order, as shown at *AA*. Let us now suppose that the electrodes, *C* and *C'*, are connected to a governable source of electromotive force and become permanently charged, *C'* positively and *C* negatively, to a certain difference of potential, tending to cause a current in the direction indicated by the arrow.

It does not require a great effort of the imagination to enable us to conceive that the first effect of the charged electrodes, *C* and *C'*, is to polarize the intervening molecules of copper sulphate by giving them a static, inductive electric charge, the copper end of each molecule being always charged positively and, therefore, always repelled from *C'* and attracted to *C*. Such charges would cause the molecules to rotate around their centers of gravity until they come into the positions shown at *BB*, whatever their previous positions might have been. These induced positive and negative charges, tending to move the two ends of the molecule in opposite directions, necessarily produce a stress opposing the force or chemical attraction holding the components of the molecule together. It is evident that when the charge reaches a certain intensity, or, when the potential difference between *C* and *C'* reaches a certain definite value, the repulsion between the oppositely charged components of the molecule will equal the affinity holding them together.

When this condition is produced, it is evident that any positively charged copper atom, c_1, is attracted equally by each of the two adjacent negatively charged atomic groups, s_1 and s_2, and that the negatively charged atomic group, s_1, is equally attracted by each of the two adjacent positively charged copper atoms, c_1 and c_2, the atom, c_2, being a part of the positively charged electrode, *C'*. The same is true of the entire series or chain of molecules reaching from *C'* to *C*, the

terminal positively charged copper atom, c_n, of the electrolyte being attracted to the adjacent negatively charged copper atom, c_m, of the electrode *C*. Any infinitesimal increase in the potential difference will now cause the charged component, c_1, to abandon its partner, s_1, and unite with the adjacent and more strongly attracting s_2, the atomic group, s_1, uniting simultaneously with c_2 of the anode, and so on throughout the series.

The succeeding state of the system is represented at *DD*, the electric charges having mutually neutralized one another, leaving the same number of molecules of copper sulphate as in the initial state. But the centers of gravity of these new molecules are all nearer to the electrode, *C'*, by half of the mean distance between two adjacent molecules. This is shown more plainly at *EE*, which represents the molecules as having rotated about their centers of gravity until they have the same relative positions as at *AA*. The newly formed copper molecule at the cathode, *C*, being firmly attached to the solid electrode, can only rotate until obstructed by that body.

After this series of changes we see that the electrode, *C*, has one more atom of copper than in the initial state, that the electrode, *C'*, has one atom less, that the number of molecules of copper sulphate is the same, but that their centers of gravity are nearer to *C'*.

These changes in concentration at the electrodes, according to the theory advanced above, are caused by the chemical changes in the electrolyte at the electrodes, that is, the introduction of copper into the electrolyte at *C'*, and its elimination from the electrolyte at *C*, in conjunction with the interchange of molecular components and the rotation of the molecules about their centers of gravity.

In regard to the energy and the electromotive force concerned in these changes, it is evident that the charging of the entire series of molecules inductively to the potential of the electrodes will require the same electromotive force, whether there are four molecules in the series or only one. We may, therefore, consider this series as a single molecule, acted upon inductively by the two electrodes, as shown in *Fig. 9*.

Whatever electrical energy may be expended in producing this inductive charge in the molecule, is evidently expended in overcoming the chemical affinity of its components. This affinity or attraction can be overcome only by restoring to the components of the molecule the chemical potential energy lost by them in combining. The attraction of the atomic group, s_1, for the copper atom, c_1, *Fig. 9*, decreases only as its attraction for c_2 increases. As s_1 acquires chemical potential energy with reference to c_1, it loses chemical potential energy with reference to c_2. That is, as s_1 passes from the influence of c_1 to that of c_2, it undergoes no actual change in its total chemical potential energy, because c_1 and c_2 are similar atoms. In other words, the

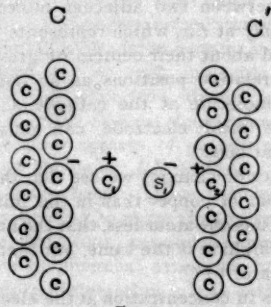

FIG. 9.

chemical affinity of s_1 is merely transferred gradually from one atom of copper to another. No energy is required for such a change and, therefore, no external electromotive force will be absorbed or evolved in this transfer of chemical affinity. This is equivalent to saying that the chemical energy generated by separating c_n (*Fig. 8*) from s_n at the cathode is equal to that which disappears by simultaneously combining s_1 with c_2 at the anode. The only external energy required is that necessary to rotate the polarized molecules about their centers of gravity and bring them into the position shown at *BB* without causing any translation. The frictional resistance of the medium to this rota-

tion would require a conversion of electrical energy into heat, that is, an electromotive force would be absorbed in overcoming the resistance of the electrolyte while the rotation continues. But the absorbed energy would be only proportional to the quantity of heat generated per unit of time, that is, to the square of the velocity of rotation. The velocity of this rotation would be proportional to the rate of electrochemical action, which, by Faraday's law, is proportional to the quantity of electric charge transported per unit of time, that is to the strength of the current. The electrical energy converted into heat by this molecular rotation must, therefore, in an electrochemical reaction, be proportional to the square of the current, corresponding to Joule's law.

It is evidently not necessary to assume that there is any transportation of matter by the electric current other than that which results from the molecular rotation described above. And whatever translation results from such rotation cannot be considered as an independent migration of dissociated ions moving with different velocities, but must be considered to result from equal velocities of rotation of the two kinds of united ions or molecular components. What is really transported or transferred from molecule to molecule directly by the current is the chemical affinity.

The chemical energy evolved or absorbed by an active electrochemical system will evidently be zero only when the chemical change at one terminal is the reversal of the chemical change at the other terminal. In all other cases there will be an evolution or absorption of electrical energy, due to the chemical changes, and a corresponding difference of potential will be developed between the electrodes. The value of this will be the sum of the electrogenic powers of the chemical changes at both electrodes, as has already been described with reference to the Daniell cell.

Faraday's Law of Electrochemical Equivalents.—The capacity of a monovalent atom or group of atoms for an electric charge sufficient to transfer its affinities is always the same, whatever may be the substance. The capacity of a divalent atom or group of atoms is always double that of a

monovalent atom, and, in general, the capacity of any atom or group of atoms is 96,540 coulombs multiplied by the change in valence suffered by the atom or group in the electrochemical action.

Stated in another form, this is Faraday's law of electrochemical equivalents.

Kohlrausch's Law of Electrolytic Conductivity.—The resistances of solutions of an electrolyte of various strengths have been very carefully determined by Kohlrausch in his classical researches. The results of his experiments on conductivity may be stated in the form of a law as follows:

The specific conductivity of an electrolyte, dissolved in a non-conducting medium, is inversely proportional to the mean distance between the molecules of the dissolved electrolyte.

This is not the form in which the results of Kohlrausch are generally stated, but it is a true statement of the relation between the strength of a dissolved electrolyte and the specific resistance of the solution. It is also a statement of what would necessarily follow from the truth of the theory of electrochemical action set forth above.

This theory of electrochemical action has not, to my knowledge, been propounded heretofore in exactly this form, though it is substantially that proposed by Faraday in 1833, and it is offered only for what it may be worth, as it seems to account satisfactorily for all changes in both energy and matter, including changes in concentration, or the so-called "migration of ions." It is also consistent with other observed facts, both physical and chemical, particularly those relating to solutions and to changes of state, temperature, and pressure. To the recent theory of "electrolytic dissociation," on the other hand, there appear to me to be insurmountable difficulties. But this subject is too large for its discussion to be entered upon here.

NOTE.—The equation on page 391 of the May number should read

$$4{\cdot}40 - (-0{\cdot}08) = 4{\cdot}48 \text{ volts.}$$

Mining and Metallurgical Section.

Stated Meeting, held Wednesday, February 13, 1901.

UTILIZATION OF THE WASTES FROM THE USE OF WHITE METALS.

BY JOSEPH RICHARDS, Member of the Institute.
(Being the Address of the Retiring President.)

(*Concluded from vol. cli, p. 455.*)

In the paper read before the section last month I treated the subject of tin clippings and zinc wastes. To-night I will speak of other wastes from the white metals, such as zinky-lead, tin-lead, antimony and its alloys, babbitt, type-metal, britannia and pewter. Also the oxides and skimmings that are taken from these metals, oxides and sulphate of lead, lead chamber wastes, "sweeps" from metallic paint works, skimmings from the pots in tin and terne-plate industries, or any other residues containing sufficient metal to pay for their reduction or refining.

Zinky-lead.—One of the first things we found it necessary to treat was zinky-lead. As I stated last month, Western spelter carries about 1½ per cent. of lead. In galvanizing, the spelter is added as often as necessary to keep up the supply in the bath. An ordinary pot will use 3 or 4 tons per week. A part of this lead sinks to the bottom of the bath, and when the bath is bailed out we obtain from 1 to 3 tons of impure lead. If this is melted in a large cast-iron pot, the molten zinc floats to the top and the lead settles to the bottom. The pot is heated just hot enough to melt the lead, and the particles of zinc, in a pasty state, float on top. A large ring about 2 feet in diameter and 6 inches deep is placed in the center of the pot. Then a ladle is taken, and by continued skimming the zinc is removed from the interior of this ring until we have a clean lead surface. The lead is then bailed out and cast into moulds or ingots.

The heat of the pot is then raised and the zinc is melted. The product is still impure, as zinc will carry about 2 per cent. lead and the lead about 3 per cent. zinc that will not settle out by this process. There is, however, considerable demand for lead for ballast, weights, counterbalances for wheels and other purposes, so that it is seldom necessary to refine the lead any closer. If we wish to do so, we have only to pass it through our refining furnace, in which the temperature may promptly be raised so high that all the zinc is volatilized and the lead becomes pure. The zinc is passed through our zinc furnace to refine in the same manner as the dross is treated.

Britannia and Pewter.—The next to command my attention was old britannia and pewter. These metals are composed of tin, lead, antimony and a little copper, varying in their composition from the English hall-marked pewter containing 92 per cent. tin, 6 per cent. antimony and 2 per cent. copper, to britannia which contains 30 per cent. tin, 6 to 10 per cent. antimony and 40 to 60 per cent. lead.

Nearly all of this metal is plated with silver, which it is necessary first to remove. We employed for this purpose the method commonly called "stripping," by plunging the articles in nitric acid contained in a porcelain-lined pot and permitting them to remain in the acid until all the silver was dissolved. The articles were well washed and dried, and then broken up and melted. The metal was used in making type, stereotype and babbitt metal. Its composition was changed, adding the desired quantity of tin, lead, antimony or copper, according to what mixture we desired to make.

The "stripping" pot was kept covered to keep the acid from evaporating, and from time to time we cleaned the old plated ware until the acid was nearly neutralized. Then common salt was added, which precipitated all the silver as chloride, which was fused with flux in a plumbago crucible to reduce it to metallic silver.

I commenced about this time to make solder for the Standard Oil Company and had considerable quantities of skimmings from the solder pots. Failing to find a purchaser for these skimmings, I proceeded to utilize them in the fol-

lowing manner: I ground and then washed them on a very fine sieve, and by this means recovered directly 50 per cent. of clean shot metal, and the oxide that was left was reduced in a crucible with carbon, yielding from 60 to 70 per cent. metal.

From that time forth I saved all my oxides for future treatment. For this purpose I built four air or pot furnaces like those used in brass melting. Their capacity was such that I soon used up all the oxides in stock, and then I systematically visited all the metal works in this and neighboring cities and found nearly all of them sieving out the shot metal to melt in their pots. The dirt (as they called it) was thrown away. I persuaded them to save the dirt for me, and while the first lot came cheaply, it soon had a market value.

We reduced the oxides in crucibles and found that the resultant metal had a very complex composition. Although it was supposed to be tin and lead only, we found mixed with it all kinds of impurities, such as iron, zinc, copper, arsenic, antimony, nickel and other metals. This necessitated the refining of the crude reduced alloys so as to get them in a marketable shape. If lead and tin predominated, 1 per cent. of iron, zinc, copper or antimony rendered it unfit for solder, the natural product of tin and lead.

These crude metals were cleaned by using sulphate of zinc to remove arsenic, sulphur to remove the antimony and superheated steam or chlorine gas to remove the iron. Salt, rosin, sal-ammoniac and other fluxes were used, leaving, after treatment, a pure alloy of tin and lead for solder making.

The crucible practice being found slow and unsatisfactory, I built one of the old-fashioned furnaces such as the Chinese use in reducing tin ore, of which we soon had three in operation.

I found that they could be run continuously and that I could add to the charge the necessary flux to take the iron and copper down in the slag. The zinc and arsenic were partially volatilized, and, if desired, also a good part of the antimony. Business grew and this method was found too

slow, so I built a water-jacketed furnace that worked much faster, the one furnace having a capacity ten times greater than that of the other three combined. Unfortunately, we did not do much work with this furnace, because of the exorbitant charge of the city water bureau ($850 a year) for the water necessary to keep it cool.

I then built a furnace of peculiar design, perfectly adapted for this use, a furnace that gave me a perfect reducing flame, absolutely under control and saving 70 per cent. in fuel.

In this furnace it was easy to treat 3,000 to 4,000 pounds at a charge. I could slag out copper and iron, volatilize other impurities and obtain a much better product. The furnace was fitted with a large condenser to cool the vapors and recover any volatile matter that might escape and annoy the neighbors.

In working this furnace the oxides are mixed according to their action in the furnace. The action of lead oxide, or litharge, on the firebrick destroys the lining of a furnace very quickly. I have known it to eat up to a thin wedge shape a 9-inch silicon firebrick in forty-eight hours. This action can be neutralized by using tin oxide and other oxides. No flux, or very little, is used and economic results are obtained.

The crude metal reduced from the oxides is liquated on an inclined hearth at a low heat, leaving copper, antimony and iron as a residue.

After liquation the metal is melted in a 10-ton pot furnace to produce a uniform mixture. When tested at this point it usually runs 25 per cent. tin, 70 per cent. lead, 1 to 2 per cent. antimony and a little iron and copper. This metal is treated in a manner similar to the zinc in the zinc furnace, using the desired reagents to remove antimony, iron and copper. The treatment is continued until the metal is pure.

Type metal oxides reduced in this manner make better solder than that made from metals found in the market.

The copper and antimony concentrates are used for babbitt metal.

Alloys of copper in excess can be used for brass.

Alloys of tin, lead, antimony can be used for type, stereotype and electrotype, or linotype metals. Lead and tin alloys are used for solder.

I will mention, in conclusion, the valuable aid that I have received in this work from the testing machine which I have devised and which I show you. It is a difficult and tedious task to determine the proportions of tin and lead in a tin and lead mixture, but with the help of this machine the result can be obtained in five minutes. These machines are now in use in all the large smelting works in this country and in Europe.

PHYSICIAL SECTION.

Stated Meeting, held March 23, 1901.

THERMOMETER GLASS AT HIGHER TEMPERATURES.

WM. MCCLELLAN, B.S.

Morgan Laboratory of Physics, University of Pennsylvania.

It is well known that an ordinary mercury-in-glass thermometer cannot be used for temperatures much over 300°, for the reason that mercury, under atmospheric pressure, boils at 357°. In addition to this, the fact that some of our hardest glass, while it does not actually become plastic, reaches a critical state at about 410°, sets a second limit. The increase in plasticity is, of course, so very slow as we pass above this point, that glass thermometers are used at considerably higher temperatures. It has always seemed desirable to widen the range of so useful an instrument by a hundred or more degrees, that is, to 450°, and the method usually adopted, is to increase the pressure on the free surface of the mercury and so raise the boiling point. The increased pressure is obtained by filling the tube, above the mercury, with nitrogen. A manufacturing firm of this city has been making this form of thermometer for commercial

purposes, chiefly for use in the metal pots of linotype machines, and has found them to work quite well. After prolonged use, however, what seemed a curious and large error developed itself. If a new and an old thermometer, both having been made of the same stock, under the same conditions, and calibrated alike, were placed in the same bath, the new one was found to read about 17° below the old one. A new and an old thermometer were brought to the University Laboratory for explanation of this error, and it was decided to submit the thermometers to a few simple tests. A description of their construction will first be given.

The bulb part is blown from a hard German glass. It is a short cylinder, about a centimeter long and 5 millimeters diameter. This is fused to a lens front tube of hard American glass. The bulb and tube are filled with mercury in the usual way and the tube drawn out for sealing. A nitrogen bag is then attached, and the bulb cooled to about 7°, allowing the gas to fill the remainder of the tube, under atmospheric pressure, after which the sealing is completed. The instrument is calibrated by comparison with a standard at 16·6° and 100° and the remainder of the scale laid off proportionally. Of course there is chance for serious error here in even approximate work. The coefficient of expansion of mercury has never been measured above 300° and to assume that the relation at low temperatures is the same for higher ones is not warranted. A much more reasonable plan, though still subject to error, would be to ascertain also a fixed point at the upper end of the scale, and then divide proportionally. In this especial case, however, the use of the instrument is largely thermoscopic, and a single mark at the temperature at which it is to be used, would answer as well. The scale is of brass, separate from the thermometer and extended only from 500° to 800° F. It is graduated in 10° spaces, each space being about 3 millimeters. It was not convenient to use this scale, however, so all readings were taken in centimeters from the top of the thermometer, and afterward changed to Centigrade degrees. There were about 27 centimeters of the tube filled with nitrogen at 76 millimeters pressure and a simple calcu-

lation shows that when the mercury column was 5 centimeters from the top, that is, at 430°, there was a pressure of 5 atmospheres. The tension of mercury vapor at 450° is 3400 mm., so that a pressure of 5 atmospheres is none too much to prevent boiling at this temperature. On the other hand Schott* of Jena made use of thermometers of this class in which the initial pressure was 10 atmospheres, and the final pressure 27 atmospheres. This, apparently, was done purposely, in order to see whether a large, even excessive internal pressure would cause an enlargement of the bulb where contraction had formerly been noticed. Wiebe† used thermometers in which the pressures had about the same values as our own. There seems no good reason for using an unnecessarily high pressure, as it has one function only, that is, to raise the boiling point. Three of these thermometers were at our disposal, which we shall refer to as Nos. I, II and III. Thermometer No. I had been in service about three weeks for six or seven hours per day at a temperature of 370°. Nos. II and III were perfectly new. All three were from the same lot, and so far as could be learned were practically identical in construction and calibration.

The glass was immediately suspected as being the seat of the trouble. The motion of the fixed points is no new phenomenon in thermometry. The slow rise of the zero point of ordinary thermometers has often been observed, and we are constantly attending to this error in accurate work stretching over any considerable time. The observations of Dr. Joule on his thermometer, covering a period of thirty-eight years, are most interesting, and are plotted in Curve I, *Fig. 1*, for comparison. It has been customary to assign, as a cause for this phenomenon, the continual external pressure, which caused the bulb to sag inwards. That this is not the case is sufficiently shown by the results obtained by Schott, with the thermometers previously mentioned. With an internal pressure of 26 atmospheres, the zero points

* Schott, *Zeit. für Ins.-Kunde*, X.
† Wiebe, *Ibid.*, XI.

rose 15° after the thermometers had been kept at 470° for two and one-half days and then at 360° for nine days. Wiebe found a rise of the zero point of 21° after a gradual heating to 420°. In this case the internal pressure was about 5 atmospheres. The effect, therefore, was not due to any weakening or softening of the glass, for, in that case, such excessive internal pressures would cause the zero point to fall rather than to rise, and would finally result in rupture. The true cause of the phenomenon has been found to be a question of internal strains. When glass or any other substance of a crystalline nature is heated to plasticity, and then allowed to cool, no matter how slowly, strains are known to establish themselves. Sudden cooling causes them to form at such a rate that fracture may result. On

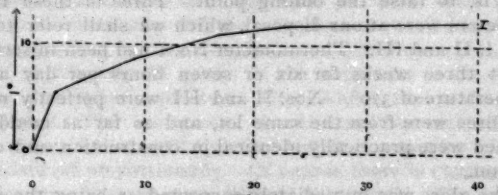

FIG. I.

the other hand a very slow cooling, that is, the ordinary process of annealing, will often cause them to disappear altogether. In glass, however, they never entirely disappear in any common process of annealing.

Makers of thermometers are accustomed to anneal their glass after blowing, and then, in order to still further eliminate the trouble, allow them to season for at least six months. In the case of Curve I, however, it will be noticed that even after a lapse of thirty years, the curve is inclining upwards appreciably. The forces of restitution are so small that a long interval is necessary for them to effect any results. If, however, we raise the temperature of the glass to a certain point, we allow the reaction to take place at a much greater rate, and this is the explanation of the phenomenon noticed with these thermometers. Moreover,

it has been shown that every glass has a certain temperature at which this increase in the rate of restitution takes place. Schott examined five different specimens of glass, two of which were designed for thermometer construction, viz: Jena Normal Thermometer Glass, and Borosilicate Thermometer Glass 59III. The analyses are given of all five, and it is noticeable that the two thermometer glasses both contain sodium and neither potassium. In fact this writer asserts elsewhere, that a glass with both sodium and potassium in it alwas shows an increased amount of change of the fixed points. He found the Normal glass softened at 400°, and the Boro 59III at 430°. This does not mean that the glass was showing any signs of plasticity, but merely that the strains were weakened, and the molecules rearranging themselves with greater freedom. The method of testing was an optical one, the glass being in the form of blocks. Later, some of the 59III was made into an air thermometer bulb, and placed in a bath which was gradually raised in temperature, in order to test the plasticity. A temperature of about 600° was reached before the pressure began to decrease steadily showing that the glass had become plastic. With glass of this sort, therefore, instruments could be made for temperatures up to possibly 550°, especially if kept at this temperature for a short time only.

THERMOMETERS I, II AND III.

The method of testing was quite simple, as nothing elaborate was desired. It was first thought desirable to have a higher temperature that could be duplicated easily, and mercury vapor was chosen. A boiler which had been prepared for other purposes was used, and gave a known temperature of 357°. Alternate readings were taken in the mercury and steam baths. This was continued until the change seemed to be greatly decreased. With No. III an air bath was used instead of the mercury. A piece of 1¼-inch iron pipe, with two Bunsen burners was used, and gave a temperature, that with a small amount of adjusting, varied less than 10° in several hours. It was thought advisable to take readings at the high temperature point, but this could only be

done when the mercury bath was used. It is interesting to note that in Thermometer No. II, the change of this point was about the same as the 100° point, showing a displacement of the whole scale. The barometer was read, and the boiling points corrected. Thermometer No. I, as has been stated, had been in service for approximately 100 hours at 370°. This was not continuous use, but at intervals of six to seven hours per day. There is little reason, however, why this should make any difference in the final effect, as the action is continuous. Little testing could be done, and but four readings were taken. The maker had etched marks at the six points 16·6°, 100°, 183·3°, 266·6°,

TABLE I—THERMOMETER I.

Date.	Temp.	Heating Interval.
Jan. 24	113·1	Ap. 100 hours at 370°
Jan. 25	114·6	1 hour at 355°
Mar. 8	115·3	½ hour at 355°
Mar. 8	115·0	8 hours at 370°

TABLE II—THERMOMETER II.

Date.	Temp.	Heating Interval.	Bath.
Jan. 24	100·	0 hours	Steam
Jan 28	349·4	30 minutes at 357° ap.	Hg.
	101·1		S.
	349·4	30 minutes " "	Hg.
Jan. 29	103·9		S.
	351·7	30 minutes " "	Hg.
Jan. 30	103·9		S.
	353·3	30 minutes " "	Hg.
Jan. 31	103·0		S.
	353·9	1 hour " "	Hg.
Feb. 1	105·0		S.
Feb. 2		4 hours " "	Hg.
Feb. 5	106·6		S.
	355·5	7 hours " "	Hg.
Feb. 20	107·7		S.
	358·8	7 hours " "	Hg.
Feb. 21	108·3		S.
Feb. 25	358·3	13 hours " "	Hg.
Feb. 26	108·3		S.
Mar. 12	375·0	8·5 hours " "	Air
Mar. 13	110·0		S.

TABLE III.—THERMOMETER III.

Date.	Temp.	Heating Interval.
Mar. 19	99·1	0 hours
	114·3	5 hours at 410°
Mar. 20	113·2	
	116·2	5 hours at 410°
Mar. 21	115·7	
	117·3	7 hours at 400°
Mar. 22	116·6	
	118·1	5 hours at 400°
Mar. 23	118·0	
	118·3	3·5 hours at 420°
Mar. 25	116·2	
	118·8	7 hours at 415°
Mar. 26	118·3	
	118·3	8 hours at 430°
Mar. 27	118·4	

349·9° and 433·2°. They were at equal distances apart, the 16·6° and 100° points being obtained by comparison with a standard, as stated. The readings taken are shown in Table I. It is necessary to remember that readings had to be taken by an auxiliary scale, and are probably accurate to a quarter of a millimeter. Now since one degree is about ·5 millimeter, we are reading to about a half degree. Therefore, the last two temperatures of Table I must be considered as showing little or no change, especially as the last one after eight hours at 370°C. shows a slight drop instead of a rise. The column "Heating Interval" means that the corresponding temperature reading had been taken after

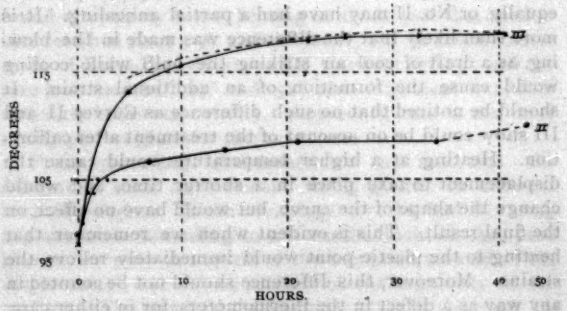

FIG. 2.

the thermometer had been kept at the high temperature for the indicated hours. The steam readings were usually taken about fifteen hours after the thermometer had cooled.

Table II shows the readings taken with Thermometer II. The steam readings are plotted in Curve II, *Fig. 2*. Though the last two readings are identical, we might confidently expect that a higher temperature and prolonged heating would show a further rise. It would be very slight, however, for the thermometer has evidently reached a nearly permanent state. Readings would have been continued had not an unfortunate accident put the thermometer out of service. The dotted portion of Curve II shows a reading

that was taken after the thermometer was broken. It shows a considerable rise, but the writer is not sure that an accurate measurement could be made on the thermometer in its broken condition. Thermometer III was obtained at this time, and was from the same lot as No. II, and the readings on this instrument are shown in Table III and Curve III, *Fig. 2*. There was no essential difference in the treatment from that of No. II except that the air bath was used entirely and the temperature of the heating bath was from 50° to 60° C. higher. This does not account, however, for the great difference in the displacements of the scales. Evidently No. III has had a different history from No. II. No. III may possibly have been cooled suddenly or unequally, or No. II may have had a partial annealing. It is more than likely that the difference was made in the blowing, as a draft of cool air striking the bulb while cooling would cause the formation of an additional strain. It should be noticed that no such difference as Curves II and III show could be on account of the treatment after calibration. Heating at a higher temperature would cause the displacement to take place in a shorter time, and would change the shape of the curve, but would have no effect on the final result. This is evident when we remember that heating to the plastic point would immediately relieve the strains. Moreover, this difference should not be counted in any way as a defect in the thermometers, for in either case, after heating, we have what is desired, a practically permanent calibration. It should be stated here that a large number of these thermometers have been made, and that the displacement is usually about 18°. No. II, therefore, shows an unusually small displacement. The dotted curve of No. III shows the readings obtained immediately after the heating, and are invariably higher, as would be expected.

These thermometers were made, as stated, of ordinary hard glass, and it is interesting to note some comparisons that have been made of this effect in several varieties of glass. According to Wiebe we have for Jena Normal Thermometer Glass a rise of the zero point of .04° in 285 days at ordinary temperature; Thüringen Glass, a former

standard German glass, showed six times as much, and English glass four times as much. On heating the three varieties for nineteen hours at 360°, the Jena thermometer rose 2·21°, and the Thüringen 7·30°, or more than three times as much. These experiments also showed that all the thermometers had a tendency towards a more or less fixed position of the scale, after prolonged heating at a high temperature. Borosilicate, 59III and Baryt-Borosilicate 122III are two other varieties of thermometer glass which are made at the Jena factories. Less information of these varieties is at hand than some of the others. Wiebe took two thermometers of 59III, one made in the ordinary way, and the other "fine annealed," and found after thirty hours' heating at about 300° that the former zero had risen 3·9°, and the latter ·2°. A thermometer made of Baryt-Borosilicate 122III was kept at 325°, and the zero had risen ·21°, ·45° and ·51° after eighteen, forty-four and sixty hours respectively. Apparently this is the most nearly perfect glass for thermometers.

From preceding statements we can see that the amount of scale displacement varies greatly in different varieties of glass. Much advantage is to be had by using a glass made according to a formula, such as the Jena glasses, not only on account of the decreased amount of the displacement, but also on account of the regularity of the product. The standard thermometer glass in Germany today is probably Borosilicate 59III, chiefly on account of its high fusing point and low coefficient of expansion. By constructing a thermometer of such a glass, we are able to predict just how, and to what extent, changes will take place. Of course there can be no such regularity in glass made in a more haphazard way, and containing materials which experiment shows to be deleterious; as, for example, the presence of sodium and potassium in the same glass. The great difference in the amount of scale displacement indicated by Curves II and III has been shown to be due, not to irregularity in the material, but more likely to a lack of uniformity in the construction. We may be certain that there are several particular operations essentially necessary

for the construction of a thermometer which is to be accurate and nearly permanent. The glass should be well selected; the empty bulb and tube after being prepared for filling should be carefully annealed by a very slow cooling process. After being filled and sealed, the thermometer should be kept at as high a temperature as convenient, and for a time depending on this temperature. For thermometers not to be used over 100°, probably the present method of seasoning is sufficient, although the seasoning would be hastened, and be more complete if allowed to take place at a higher than room temperature. For high-temperature thermometers it is essential that they be kept at about 450° for three or four days. This is comparatively easy, and cheap if an air bath be used, and would obviate the difficulty which has been described. Thermometers after such treatment could be calibrated for high temperatures and be expected to change little during a long period of time. A volume written by Hovestadt on Jena glass no doubt contains much information on this subject, but the writer saw the book too late to do more than glance through it.

OBITUARY.

DAVID SHEPHERD HOLMAN.

David Shepherd Holman, who was Actuary to the Franklin Institute from 1871 to 1885, died at Bangor, Me., on May 13, 1901.

Mr. Holman was born in Milo, Me., in 1826, and at the time of his death was in his seventy-fifth year. He became identified with the Institute as a member in 1866, and at once took an active interest in its work. Although he was to a great extent self-educated, and had not enjoyed the advantages of early scientific training, he succeeded in becoming a skillful microscopist, and, being gifted with great mechanical ingenuity, devised a number of useful microscopic accessories, with some of which his name is associated. Of these, perhaps, the best known are the so-called "Holman life-slides," which he devised for the pur-

pose of facilitating the study of the vital functions in living animals. He also originated other devices and methods for illustrating the circulation of the vital fluids in animals and plants, which gained for him considerable local repute.

He was mainly instrumental in the foundation of the Optical Section of the Franklin Institute, in the year 1872, the first Section to be founded in the Institute. The force of his personal enthusiasm gave to this Section a notable career for several years.

Mr. Holman was among the first to foresee and appreciate at their true value the great advantages of the typewriting machine at the time of its first introduction, and when the future of this great time- and labor-saving device was still problematical. From the beginning, his faith in the universal applicability of the typewriter, with the supplementary aid of stenography, was unbounded. He gave practical form to his ideas on this subject by founding, in 1873, at the Institute, a school of stenography and typewriting, which soon was largely attended and most successful. All this seems but a commonplace matter from the point of view of to-day, but the local stimulus that Mr. Holman's pioneer work gave to the modern art of correspondence was unquestionably of great value, and he is entitled to the fullest recognition for this service.

After the severing of his official connection with the Franklin Institute in 1885, Mr. Holman became associated with the Atlantic Refining Company, and for a number of years was in charge of a laboratory which he equipped for testing the viscosity and other physical properties of mineral oils. While in this position he devised a "viscosi-

meter," known by his name, which depended for its operative feature on the time required for the descent of a metal sphere through a column of given height of the oil under examination.

Later, Mr. Holman was appointed instructor in stenography and typewriting in Girard College, which position he occupied for several years. He also became actively interested in the Academy of Natural Sciences of Philadelphia. For a number of years, also, he was in the lecture field, where his skill as a demonstrator with the projecting microscope enabled him to achieve considerable success as a teacher and experimenter.

During his official connection with the Franklin Institute, extending over fifteen years, Mr. Holman's obliging disposition and unvarying amiability made him many friends, who will sincely mourn his loss.

LOUIS E. LEVY,
WM. H. WAHL,
H. R. HEYL,
Committee.

COMMITTEE ON LIBRARY.

Stated Meeting, held Monday, June 10, 1901.

The Librarian reported the following additions to the library during the month of May:

	Bound vols.	Unbound vols.	Pamphlets.
By gift	32	10	91
" purchase	2	—	—
From the JOURNAL	2	5	—
	36	15	91

Total additions for the month 142

Gifts were reported from the Ammonia Co. of Philadelphia; Dr. H. Kammerlingh-Onnes of Leyden, Holland; Dr. Wm. H. Greene; Messrs. Samuel Sartain, C. W. Swoope and Lewis S. Ware. The usual monthly receipt of United States Government publications and exchanges was reported.

W.

NOTES AND COMMENTS.

POWER FROM BLAST-FURNACE GAS.

The *Iron Age* makes the following interesting editorial comments on the progress made in Germany in connection with the utilization of blast-furnace for power as described in a recent paper, by Herr F. W. Luermann, before the *Verein Deutscher Eisenhüttenleute*, viz.: Herr Luermann, after some incidental reference to the apparent inactivity in this field of American engineers, records the fact that furnace gas engines to the extent of 77,545 horse-power have been delivered or are under construction in Europe, Germany being credited with 44,665 horse-power, while Great Britain is in the list with 600 horse-power. The editor believes, however, that the apathy of American furnace managers is much more apparent than real. Engine builders and iron makers are somewhat mysterious as to what they are really doing. Suffice it to say that our engineers are not by any means satisfied with the designs brought forward, particularly in blowing engines, and that quietly a good deal of work is being done.

Luermann has been doing splendid work as a recorder of current progress and his latest paper, the third of the series, brings developments close up to date. His reports are all the more interesting and valuable since he is not in any way connected with any make of gas engine, and occupies the position of an independent and a frank critic.

At the outstart Luermann took the ground that the furnace gas must first be thoroughly cleaned not only of dust, but also of metal vapors and of steam. He draws the conclusion from the experience gathered in practice thus far that fluctuations in the composition of blast-furnace gas have no appreciable effect, and further, that the low calorific value of some gases is no serious obstacle, a Koerting engine, for instance, being operated at the Mansfeld copper smelter at Eisleben with gas having only 700 heat units per cubic meter.

The cleaning of the gas, however, is now acknowledged to be a vital point. Luermann describes the Kloenne washer, the Theisen centrifugal machine, the plant at Frieden, Gutehoffnung, and Georg-Marie works, which are pronounced the most elaborate in Germany. At Differdingen the first catching and cooling installation proved inadequate and the records show that the blowing engines did not run as steadily as the gas engines used for operating dynamos.

Luermann also describes the different types of gas engines in use, the Seraing, the Oechselhaueser, the Koerting and the Nuernberg. The question of water cooling the piston is one which seems to have come forward lately, and the conclusion has apparently been reached that up to a certain size of cylinder, such precautions are not necessary.

It seems that designers are also turning to the question of reducing the waste of gas at the tunnel head by more elaborate and efficient arrangements.

Luermann reports that data relative to the consumption of gas in engines are not yet numerous and vary from 2·56 centimeters to 3·67 centimeters per

horse-power. With the high cost of coke in Germany, the saving figures out about $1.50 per ton of pig iron produced, which is certainly a very tempting sum.

There can be no doubt but what the utilization of the waste gas from the blast-furnace for the operation of blowing engines, for the production of electric power, and possibly for the manufacture of calcium carbide, has proved practical. While our lower fuel cost does not hold out so large a saving as that realized in Europe, and while the powdery ores of many of our leading districts threaten to give us special trouble, it seems absolutely necessary that our gas engine builders on the one side and our furnacemen on the other, must soon seriously attack the problem of adapting these improvements to our special conditions.

BOOK NOTICES.

Elaboration des Metaux dérivés du Fer. Réactions métallurgiques. Par L. Gage, Capitaine d'Artillerie. Petit in-8, avec 17 figures (*Encyclopédie scientifique des Aide-Mémoire*). Broché, 2 francs 50 cent.; Cartonné, 3 francs.

Elaboration des Metaux derives du Fer. Foyers métallurgiques. Par L. Gages, Capitaine d'Artillerie. Petit in-8, avec 22 figures (*Encyclopèdie scientifique des Aide-Memoire*). Broché, 2 francs, 50 cent.; Cartonné, 3 francs. Paris: Libraire Gauthier-Villars. 1901.

These volumes constitute the latest additions to the Encyclopédie Scientifique des Aide-Mémoire, of which many previous numbers of the series have been noticed in this column.

The volume first named is devoted to the subjects of fuels, natural and artificial, furnaces of various types and refractory materials.

The second volume is devoted to the various procedures for the production of iron and steel. Each volume of this series is complete in itself.

W.

Atoms and Energies. By D. O. Murray, A.M., Sometime Instructor in the Government Shogyo Gakko, Kyoto, Japan. 8vo, pp. 200. New York: A. S. Barnes & Co., 1901. (Price $1.25.)

The author seeks in this work to establish the probability of certain views respecting the nature of matter and energy radically different from those generally accepted. In Chapter I he starts with the proposition that the terms Large and Small are merely terms of comparison, not abstract dimensions. An atom, consequently, must be very large as compared with some other size. He maintains as a consequence of this assumption, that atoms in solids are in actual contact. In Chapter II, the author considers the question of the shapes of atoms, and reaches the conclusion that the determining effect upon their interactions and relations is conditional upon their size and shape. Chapter III deals with the subject of chemical affinity, in which the ground is taken that differences in shape suffice to account for all the phenomena. Chapter V seeks to identify the simple energy that produces the results of chemical affinity, with the attractive energies known as cohesion, adhesion and gravitation. Chapter VI discusses expansive energy; Chapter

VII, electricity and magnetism; Chapters VIII and IX, the three states of matter; Chapter X, vibration. Chapter XI concludes the work, and in this it is sought to be shown that energy cannot be considered merely as a mode of motion, that it is not merely an attribute of matter, but is a distinct entity.

W.

Select Methods in Food Analysis. By Henry Leffmann, A.M., M.D., and William Beam, A.M., M.D. 8vo, pp. 383. Philadelphia: P. Blakiston's Son & Co., 1901.

"This book is intended to be a concise summary of the analytic methods adapted to the needs of both practising analysts and advanced students in applied chemistry," and it must be admitted that this aim has been fully attained by the authors.

The principal methods of food analysis, especially those practised in American and English laboratories, are well selected and carefully described. The arrangement, too, is excellent.

Sixty-seven pages of the book are devoted to the description of general or more frequently recurring physical and chemical determinations, and these are followed by a very interesting chapter on the detection of poisonous metals, coloring matters, and preservatives. The special methods employed in the examination of the various articles of food constitute the bulk of the work. This includes the following subjects—starch and materials used in bread-making, fats and oils, milk and milk products, non-alcoholic beverages, condiments and spices, alcoholic beverages, and flesh foods.

While the description is concise, it appears to contain all that is essential to the obtaining of good results by an expert analyst, and, inasmuch as many data and processes from official sources (*e. g.* the United States Department of Agriculture and the Association of Official Agricultural Chemists) are made more accessible to chemists than are the original bulletins, we venture the prediction that this volume will soon be found within easy reach on the book-shelves of every English-speaking food analyst.

H. F. K.

Induction Coils: How to make, use and repair them. Including Ruhmkorff, Tesla and medical coils, Roentgen radiography, wireless telegraphy and practical information on primary and secondary batteries. By H. S. Norrie (Norman H. Schneider), second edition, revised and much enlarged. 12mo, pp. 269. New York: Spon & Chamberlain; London: E. & F. N. Spon. (Price, cloth, $1.00.)

The author explains that his inducements to prepare a second edition of his work were, in part, the great favor with which the first edition was received, and partly the steadily growing interest in the subject which has been manifested in the scientific activity and numerous improved forms of apparatus that have been made known within the past few years. The author in his endeavor to bring the volume up to date, has thoroughly revised it, partly rewritten it, and added considerable new matter with numerous new illustrations.

Considerable space in the new matter is given to the following: Medical and bath coils, gas engine and spark coils, contact-breakers, primary and

secondary batteries, electric gas lighting, new methods of X-ray work and a complete chapter on wireless telegraphy. A complete index, contents, list of illustrations and contents of tables have been added. W.

Les Discharges Electriques dans les Gaz. Par J. J. Thomson, P.S.C., F.R.S. Ouvrage traduit de l'Anglais avec des Notes, par Louis Barbillion, Dr. es Sc., et un preface par Ch. Ed. Guillaume. Paris: Gauthier-Villars 1900.

This translation of Dr. Thomson's "Electrical Discharges in Gases" should be readily appreciated by French readers. The work is well known and esteemed as a scientific classic by English students of electrical science.

W.

Elektrische Verbrauchsmesser der Neuzeit für den praktischen Gebrauch dargestellt von Johannes Zacharias, Ingenieur. Mit 194 Abbildungen im Text und zahlreichen Tabellen. Halle a/S. Verlag von Wilhelm Knapp, 1901. (R.M., 15.)

The literature relating to electric meters is extremely meagre, and the present work will undoubtedly prove of much value to electrical engineers. The compilation of patent data appended to the descriptive portion of the work is a feature which will be highly appreciated by all who have occasion to look into the history of the subject. W.

Recueil de Problèmes de Géomètrie Analytique a l'usage des Classes de Mathématiques Spéciales. Solutions des Problèmes donnés aux Concours d'admission à l'École Polytechnique de 1860 a 1900. Par François Michel, Ancien Élève de l'École Polytechnique, Inspecteur de l'Exploitation aux Chemins de fer du Nord. Un volume in-8, avec 60 figures. 1900. 6 francs. Paris: Librairie Gauthier-Villars.

This volume contains the problems and their solution which had been given to the candidates for admission in the *École Polytechnique* from the years 1860 to 1900 inclusive. W.

Franklin Institute.

[*Proceedings of the stated meeting held Wednesday, June 19, 1901.*]

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, June 19, 1901.

President JOHN BIRKINBINE in the chair.

Present, 84 members and visitors.

Additions to membership since last month, 12.

The special committee named at the previous meeting, to prepare a memorial of the late David Shepherd Holman, one time Actuary of the Franklin Institute, presented a report, through its Chairman, Mr. L. E. Levy, which was accepted and the committee discharged with thanks.

Prof. Lewis M. Haupt presented a communication entitled, "Obstructions to Commerce." The speaker discussed in a general way the various natural impediments to free commercial intercourse, dwelling especially upon the obstructious offered by continental barriers, and harbor entrances. The subject was profusely illustrated with the aid of lantern slides, and is reserved for publication in full. Discussed by Messrs. L. E. Levy, the President and the author.

Mr. Edwin S. Church, superintendent of machinery of the U. S. Mint at Philadelphia, presented an account, freely illustrated with lantern slides, of the engineering equipment of the New Mint, discussed by Professor Haupt, Messrs. O. E. Outerbridge, Washington Jones, Sam'l R. Marshall and the author. (Referred for publication.)

Adjourned.

WM. H. WAHL,
Secretary.

COMMITTEE ON SCIENCE AND THE ARTS.

[Abstract of proceedings of the stated meeting held Wednesday, June 5, 1901.]

The following reports were adopted:

(No. 1792.) *Electric Liquid Healing Process.*—Geo. D. Burton, Boston, Mass. Dismissed.

(No. 1992.) *System of Oil Heating and Incandescent Lighting.*—Arthur Kitson, Philadelphia.

ABSTRACT.—This invention is the subject of numerous patents granted to applicant, dating from 1888 to 1899, copies of all of which are filed with the papers of the case. The report gives an exhaustive account of the history of the art, which may be summarized in the following abstract from the same: The application of Mr. Kitson in this case was for an award to him as the original inventor of a lamp and system for burning kerosene oil under a mantle to produce an incandescent light, the oil being under suitable pressure and vaporized at the lamp before being burned. After a very careful and thorough study of the facts, the Sub-Committee finds it impossible to make an award to Mr. Kitson as the original inventor of such a lamp and system. The Sub-Committee does believe it proper, however, to recognize the merit which is shown in the perfection of a lamp which satisfactorily vaporizes and uses kerosene oil for incandescent mantle lighting and its commercial success in competition with other artificial illuminants, and, therefore, recommends the award to the inventor of the Edward Longstreth Medal of Merit. [*Sub-Committee*—Arthur J. Rowland, Chairman; Chas. A. Hexamer, L. L. Cheney, Geo. A. Hoadley, Geo. F. Stradling, Wm. McDevitt, Frank P. Brown.]

(No. 2134.) *Improvements in Musical Instruments.*—Chas. F. Albert, Philadelphia. (Referred by the Jury of Awards, National Export Exposition.)

ABSTRACT.—This report refers to the fact that the general excellence of the musical instruments made by Mr. Albert has already been recognized by

the grant of the highest award in the gift of the Institute. The exhibit, however, embraced one invention of importance which had not previously been brought to the Committee's attention, namely, Mr. Albert's triple-covered flexible G strings for violins, violos and cellos. This invention consists in covering gut or other strings for musical instruments with silk thread or its equivalent, and in covering the thread-wrapped string with two wires, whereby certain positive advantages are secured. There were other minor features included in the invention.

The serious defect in all similarly wrapped strings lay in the fact that the drying of the gut core caused the loosening of the single wire wrapping, so that it would roll under the friction of the bow, and rattle, thus destroying its tone qualities. This and other defects, it is admitted, are corrected by Mr. Albert's invention, and his triple-wound G strings have come into general use. The award of the John Scott Legacy Premium and Medal is recommended. [*Sub-Committee.*—H. R. Heyl, Chairman; Wm. Stoll, Jr., Chas. M. Schmitz, Paul Sentz, Richard Zeckwer, Henry C. Wilt.]

(No. 2175.) *Balance for Testing White Metal Alloys.*—Joseph Richards, Philadelphia.

This report is reserved for publication in full. The award of the John Scott Legacy Premium and Medal is recommended. [*Sub-Committee.*—H. F. Keller, Chairman; F. Lynwood Garrison, G. H. Clamer.]

(No. 2178. *Automatic Micrometer Rolling Mill Gauge.*—Robt. B. Haines, Philadelphia.

A re-investigation, The award of the John Scott Legacy Premium and Medal is recommended in view of the fact that the invention having come into universal use in the United States, and to some extent also in Europe, has demonstrated high utility for its intended service. [*Sub-Committee.*—James Christie, Chairman; Thos. P. Conard, A. Falkenau.]

The following reports passed first reading:

(No. 2151.) *Tobacco Cutting Machines.*—John B. Adt, Baltimore, Md.

(No. 2148.) *Damper Regulator.*—N. C. Locke, Salem, Mass.

Made advisory and adopted. The reason assigned in the report is that applicant has presented no new data.

(No. 2179.) *Combustion Crucible,*—Porter W. Shimer, Easton, Pa.

(No. 2188.) *Electrolytic Production of Metals and Nitric Acid from Fused Nitrates.*—James D. Darling, Philadelphia.

(No. 2113.) *Liszt Organs.*—Mason & Hamlin Company, Boston, Mass, Referred back to the Sub-Committee for amendment.

The following cases were dismissed:

(No. 1859.) *Safety Device for Elevators.*—S. D. Strohm, Philadelphia, Pa.

(No. 1883.) *High Tension Storage Battery.*—N. H. Edgerton, Philadelphia.

(No. 1971.) *Synchronograph.*—Geo. O. Squier and Albert C. Crehore, Fort Monroe, Va.

(No. 2080.) *Sterilizing Apparatus.*—Jas. E. Thomas and Elisha P. Grow, Philadelphia.

(No. 2104.) *Gas and Oil Heater.*—Joshua J. Nix, Royersford, Pa.

(No. 2176.) *Typewriter.*—W. T. Hoofnagle, New York, N. Y.